

‘THE LORD LOOKS AT THE HEART’ (1 SAM 16,7)

11QPs^a 151A-B as a ‘Variant Literary Edition’ of Ps 151 LXX*

Summary

Basing itself on an earlier translation-technical examination of the two extant textual witnesses of *Ps* 151, the present study aims at further investigating the interrelation of *Ps* 151 LXX and *Ps* 151A-B. In this respect, it will examine whether a pattern of textual variants can be isolated in *Ps* 151A, in order to substantiate the claim that *Ps* 151 LXX and *Ps* 151A-B represent ‘variant literary editions’. The present contribution observes that three units of ‘pluses’ immediately stand out in *Ps* 151A and that, when taken together, they remarkably exhibit a *midrash* to 1 Sam 16,7. While *Ps* 151 LXX is only a ‘paraphrase’ of the events recounted in 1 Sam 16-17 in autobiographical style, a creative scribe has enriched its *Vorlage* with a *midrash* in *Ps* 151A, emphasising that God does not judge man according to his outward appearance, but according to his inner dedication, as it is exemplified in the preference given to David over his brothers. Most likely, both editions of *Ps* 151 are not directly connected to each other, but ultimately depend on a common ancestor, which are here termed ‘edition *n*’, indicating that this edition was not necessarily the first edition of this psalm. As both editions were derived independently from this now lost ‘edition *n*’, they are designated as ‘edition (*n*+1)^a’ and ‘edition (*n*+1)^b’.

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UNDOUBTEDLY, one of the most exciting discoveries in Qumran has been the unearthing of *11QPs^a* (*11Q5*), as this ‘Great Psalms Scroll’ has thrown striking new light on the composition of the book of Psalms. Moreover, the scroll also included a remarkable, yet considerably longer parallel to *Ps* 151, which until then was only known from Greek and Syriac Psalters. Emulating on an earlier translation-technical investigation of the textual witnesses of *Ps* 151, (1) the present paper will further explore the textual history of this particular composition. Firstly, it will offer a state of the question concerning the status of *11QPs^a*, particularly paying attention to the ‘variant literary editions’ of the Psalter as outlined by E. Ulrich and P. Flint. Secondly, several scholarly solutions to the interrelation of *Ps* 151 LXX and *Ps* 151A-B will be examined. Against this background, my own proposal will be presented, i.e. to consider them as ‘variant literary editions’ to each other (2). Thirdly, an alternative outlook on the problem will be developed, explaining the ‘pluses’ in *Ps* 151A as a *midrash* to *1 Sam* 16,7 and hence establishing that *Ps* 151A and *Ps* 151 LXX are truly ‘variant literary editions’ of the same composition.

I. The Status of *11QPs^a* as a Textual Witness to the Psalter

As is commonly known, *11QPs^a* has caused a major shift in our understanding of the composition and fixation of the Psalter, even though most scholars did not immediately realise the importance of the manuscript. In the early sixties, the editor of the critical edition of this scroll, J. Sanders, stood in a minority when he claimed that it represented a genuine form of the Psalter just as Jewish as the Masoretic collection of psalms, testifying to the complex history of canonisation of the Psalter. (3) His hypothesis was particularly rejected in the journal *Textus* of the following year by S. Talmon, who contended that the scroll was meant as a supplement to Scripture. (4) This was complemented by M. Goshen-Gottstein, who considered the scroll as the prototype of a liturgical collection of psalms taken from the already closed (proto-)Masoretic Psalter. (5) P. Skehan, too, endeavoured to

(1) See H. Debel, “Amalgamator or Faithful Translator? A Translation-Technical Assessment of Psalm 151,” in *The Composition of the Book of Psalms. Proceedings CBL 2008*, ed. E. Zenger, (BETL, Leuven: Peeters, forthcoming).

(2) Using the terminology of Ulrich, see notes 13 and 14 below.

(3) See J.A. Sanders, “Variorum in the Psalms Scroll (11QPs^a),” *HTR* 59 (1966): 83-94 (esp. pp. 89, 93) and Id., *The Dead Sea Psalms Scroll* (Ithaca, NY: Cornell University Press, 1967), 7-8.

(4) S. Talmon, “Pisqah Be’emsa’ Pasuq and 11QPs^a,” *Textus* 5 (1966): 11-21.

(5) M. Goshen-Gottstein, “The Psalms Scroll (11QPs^a). A Problem of Canon and Text,” *Textus* 5 (1966): 22-33. Be it noted that S.B. Gurewicz, “Hebrew Apocryphal Psalms from Qumran,” *ABR* 15 (1967): 13-20, pp. 14-15 too maintained that the scroll represented an early attempt to establish a prayer-book.

substantiate the secondary status of the scroll by classifying it as a 'library edition' of psalms attributed to David, arranged after the fixation of the Masoretic Psalter. (6) Likewise, the dependence of *11QPs^a* on the canonical Psalter has been argued by B. Wacholder, who suggested that the scroll presents an eschatological collection of praise attributed to David. (7) In more recent times, M. Haran and L. Schiffman too have adopted the view that *11QPs^a* merely represents a collection of psalms for liturgical use, (8) while E. Tov believes that its purpose as a prayer-book is confirmed by its being written in what he has termed 'the Qumran practice'. (9) Hence, he also advocates a more neutral designation of the scroll, as its present name suggests that it is a copy of the biblical book of Psalms. (10) Finally, the secondary nature of the scroll has lately been demonstrated by U. Dahmen, who considers *11QPs^a* as a Qumran-specific, Davidic collection of psalms dependent on the Masoretic Psalter. (11)

However, as Qumran studies have tremendously advanced in the last decades of the past century, our idea of the emergence of a standard text and the growth of the biblical canon has changed. In this regard, E. Ulrich has put forward the concept of 'variant literary editions', observing that categories such as 'standard text', 'canon' and 'bible' are, in fact, anachronistic terms for describing the history of the biblical text in the period of the Second Temple. (12) He suggests

(6) P.W. Skehan, "A Liturgical Complex in 11QPs^a," *CBQ* 34 (1973): 195-205 and Id., "Qumran and Old Testament Criticism," in *Qumrân. Sa Piété, Sa Théologie Et Son Milieu*, ed. M. Delcor (BETL 46, Paris - Gembloux: Duculot; Leuven: University Press, 1978), 163-182.

(7) B.Z. Wacholder, "David's Eschatological Psalter: 11QPsalms^a," *HUCA* 59 (1988): 23-72, esp. p. 72.

(8) M. Haran, "11QPs^a and the Canonical Book of Psalms," in *Minhah Le-Nahum*, ed. M. Brettler and M. Fishbane (JSOT SS 154, Sheffield: Academic Press, 1993), 193-201, and L.H. Schiffman, *Reclaiming the Dead Sea Scrolls. The History of Judaism, the Background of Christianity, the Lost Library of Qumran* (Philadelphia, PA - Jerusalem: Jewish Publication Society, 1994), 169.

(9) E. Tov, "Excerpted and Abbreviated Biblical Texts from Qumran," *RevQ* 16/64 (1995): 581-600, p. 595.

(10) *Ibid.*, 593. The same argument had already been put forward by Goshen-Gottstein, "The Psalms Scroll," 32.

(11) U. Dahmen, *Psalmen- und Psalter-Rezeption im Frühjudentum. Rekonstruktion, Textbestand, Struktur und Pragmatik der Psalmenrolle 11QPs^a aus Qumran* (STDJ 49, Leiden - Boston: Brill, 2003), esp. 313-318.

(12) As Ulrich aptly remarks in "Pluriformity in the Biblical Text, Text Groups, and Questions of Canon," in *The Madrid Qumran Congress. Proceedings of the International Congress on the Dead Sea Scrolls. Madrid 18-21 March 1991*, ed. J. Trebolle Barrera and L. Vegas Montaner (STDJ 11, Leiden - New York - Köln - Madrid: Brill; Madrid: Editorial Complutense, 1992), 23-41, p. 32: "If we try to achieve a historical perspective on the text of the Bible, the first step is not to talk about a Bible". In the same vein, he writes in "The Canonical Process, Textual Criticism, and Latter Stages in the Composition of the Bible," in *Sha'arei Talmon. Studies in the Bible, Qumran and the Ancient Near East*, ed. M. Fishbane and E. Tov (Winona Lake, IN: Eisenbrauns, 1992), 267-291, p. 267: "'The canon' as such is a post-bibli-

that literary units – be it books or pericopes – were often intentionally reworked, resulting in two or more parallel forms. Though the text has been standardized after the Second Jewish Revolt and variant editions have only reached us, modern scholars, by chance, Ulrich is convinced that the text was truly pluriform and still developing until the end of the first century C.E. or even well into the second century. (13) As for the Psalter, Ulrich rejects the idea that *11QPs^a* is a secondary collection of psalms and classifies it as a ‘variant literary edition’ of the Masoretic Psalter. (14) Among other things, he points out

cal topic”. Finally, in “A Qualitative Assessment of the Textual Profile of 4QSam^a,” in *Flores Florentino. Dead Sea Scrolls and Other Early Jewish Studies in Honour of Florentino García Martínez*, ed. A. Hilhorst, E. Puech, and E. Tigchelaar (JSJS 122, Leiden - Boston: Brill, 2007), 147-161, p. 150, he denies the existence of a “‘standard text’ according to which a manuscript could be ‘aligned’ or ‘non-aligned’”.

(13) Ulrich defines ‘variant literary editions’ as follows in “The Canonical Process,” 278: “By multiple literary editions I mean a literary unit – a story, pericope, narrative, poem, book, etc – appearing in two or more parallel forms (whether by chance extant or no longer extant in the textual witnesses), which one author, major redactor, or major editor completed and which a subsequent redactor or editor intentionally changed to a sufficient extent that the resultant form should be called a revised edition of that text.” For his concept of ‘variant literary editions’, reference should also be made of the following contributions of his hand: “Double Literary Editions of Biblical Narratives and Reflections on Determining the Form to Be Translated,” in *The Dead Sea Scrolls and the Origins of the Bible* (Grand Rapids, MI: Eerdmans, 1999), 34-50, p. 35 (I did not have access to the original in *Perspectives on the Hebrew Bible. Essays in Honor of Walter J. Harrelson*, ed. J.L. Crenshaw [Macon, GA: Mercer University Press, 1988] 101-116); “Pluriformity,” 25-37; “Multiple Literary Editions. Reflections toward a Theory of the History of the Biblical Text,” in *Current Research and Technological Developments on the Dead Sea Scrolls. Conference on the Texts from the Judean Desert, Jerusalem, 30 April 1995*, ed. D.W. Parry and S.D. Ricks (STDJ 20, Leiden - New York - Köln: Brill, 1996), 78-105, pp. 88-91; “The Bible in the Making: The Scriptures at Qumran” in *The Community of the Renewed Covenant. The Notre Dame Symposium on the Dead Sea Scrolls*, ed. E. Ulrich and J.C. VanderKam (CJAn 10, Notre Dame, IN: University of Notre Dame Press, 1994), 77-93, pp. 83-85; “The Dead Sea Scrolls and the Biblical Text,” in *The Dead Sea Scrolls after Fifty Years*, ed. P.W. Flint and J.C. VanderKam (Leiden - Boston - Köln: Brill, 1998), 79-100, pp. 82-86; “The Scrolls and the Study of the Hebrew Bible,” in *The Dead Sea Scrolls at Fifty. Proceedings of the 1997 Society of Biblical Literature Qumran Section Meetings*, ed. R.A. Kugler and E.M. Schuller (SBL EJL 15, Atlanta, GA: Scholars Press, 1999) 31-41, pp. 31-37; “The Dead Sea Scrolls and Their Implications for an Edition of the Septuagint Psalter,” in *Der Septuaginta-Psalter und seine Tochterübersetzungen. Symposium in Göttingen 1997*, ed. A. Aejmelaeus and U. Quast (MSU XXIV, Göttingen: Vandenhoeck & Ruprecht, 2000), 323-336, pp. 325-326; “The Qumran Biblical Scrolls - the Scriptures of Late Second Temple Judaism,” in *The Dead Sea Scrolls in Their Historical Context*, ed. T.H. Lim (Edinburgh: T&T Clark, 2000), 67-87, pp. 84-87; “The Dead Sea Scrolls and the Hebrew Scriptural Texts,” in *The Hebrew Bible and Qumran*, ed. J.H. Charlesworth, (The Bible and the Dead Sea Scrolls 1, Richland Hills, TX: Bibal, 2000), 105-133, pp. 127-128; “Our Sharper Focus on the Bible and Theology Thanks to the Dead Sea Scrolls,” *CBQ* 66 (2004): 1-24, pp. 16-17 and “A Qualitative Assessment,” 150-151.

(14) See, e.g., Ulrich, “Multiple Literary Editions,” 100-101; *Id.*, “The Bible in the Making,” 90-91; *Id.*, “The Community of Israel and the Composition of the

(1) that any form of the Psalter is by its very nature a liturgical collection, (2) that virtually all biblical books were secondary in a way, (3) that the prose composition in *11QPs^a* – the text called ‘David’s Compositions’ (col. xxvii) – includes an explicit claim to scriptural status and may originally have been a colophon at the end of the scroll, (4) that especially the Syriac Psalter also contains some additional compositions, three of which are also found in their Hebrew form in *11QPs^a*, and (5) that in other books too, especially in the case of Jeremiah, two different editions of the text have been preserved, of which the Masoretic Text is the secondary and revised one. (15)

Ulrich’s alternative view on *11QPs^a* has been elaborated by P. Flint, who has concluded on the basis of a thorough examination of all the Psalm manuscripts from the Judean Desert that the fixation of the Psalter comprised different stages and is hence represented by three major collections of Psalms. (16) Edition I, an early edition of the Psalter (though not necessarily the first grouping of psalms), included *Psalms* 1 or 2 to 89 and was later joined with other psalms in two different subsequent editions: Edition IIa, as found in the Qumran scrolls *11QPs^a*, *11QPs^b* and *4QPs^e*, (17) and Edition IIb, rep-

Scriptures,” in *The Quest for Context and Meaning. Studies in Biblical Intertextuality*, ed. C.A. Evans and S. Talmon (Biblical Interpretation Series, Leiden - New York - Köln: Brill, 1997), 327-342, p. 340; *Id.*, “The Dead Sea Scrolls and the Biblical Text,” 92-94; *Id.*, “The Scrolls and the Study of the Hebrew Bible,” 37-38; *Id.*, “The Dead Sea Scrolls and Their Implications for an Edition of the Septuagint Psalter,” 332-333; *Id.*, “The Qumran Scrolls and the Biblical Text,” in *The Dead Sea Scrolls. Fifty Years after Their Discovery. Proceedings of the Jerusalem Congress, July 20-25, 1997*, ed. L.H. Schiffman, E. Tov, and J.C. VanderKam (Jerusalem: The Israel Exploration Society, 2000), 51-59, pp. 55-56; *Id.*, “The Dead Sea Scrolls and the Hebrew Scriptural Texts,” 118-119; *Id.*, “The Text of the Hebrew Scriptures at the Time of Hillel and Jesus,” in *Congress Volume: Basel 2001*, ed. A. Lemaire (SVT 92, Leiden: Brill, 2002), 85-108, p. 103; *Id.*, “The Absence of ‘Sectarian Variants’ in the Jewish Scriptural Scrolls Found at Qumran,” in *The Bible as Book. The Hebrew Bible and the Judaean Desert Discoveries*, ed. E.D. Herbert and E. Tov (The Bible as Book 4, London - New Castle - Grand Haven: British Library, 2002), 179-195, p. 186 and *Id.*, “Our Sharper Focus,” 7-8.

(15) See, e.g., Ulrich, “Multiple Literary Editions,” 100-101; *Id.*, “The Dead Sea Scrolls and the Biblical Text,” p. 92; *Id.*, “The Dead Sea Scrolls and the Hebrew Scriptural Texts,” 119 and *Id.*, “The Text of the Hebrew Scriptures,” 103-104.

(16) See particularly his doctoral dissertation, published as P.W. Flint, *The Dead Sea Psalms Scrolls and the Book of Psalms* (STDJ 17, Leiden: Brill, 1997) and, in addition, his contributions “The Book of Psalms in the Light of the Dead Sea Scrolls,” *VT* 48 (1998): 453-472, pp. 461-464 and “Psalms and Psalters in the Dead Sea Scrolls,” in *The Hebrew Bible and Qumran*, ed. J.H. Charlesworth (The Bible and the Dead Sea Scrolls 1, Richland Hills, TX: Bibal, 2000), 307-359, pp. 317-320. However, Flint’s arguments have been called into question by H-J. Fabry, “Der Psalter in Qumran,” in *Der Psalter in Judentum und Christentum*, ed. E. Zenger, (HBS, Freiburg - Basel - Wien - Barcelona - Roma - New York: Herder, 1998), 137-163, pp. 156-159.

(17) Though confirming that *11QPs^b* is a copy of the same edition, Dahmen contends that *4QPs^e* is by no means a further parallel to *11QPs^a*. See Dahmen, *Psalmen- und Psalter-Rezeption*, 50-59.

resented in the Masoretic Psalter, which is clearly supported by *MasPs^b*, and possibly by other Qumran Scrolls too. In this respect, however, the documentary evidence is too fragmentary to draw hard-core conclusions, as most of the scrolls can be arranged in order to support either Edition IIa or Edition IIb. There may have been further editions of the Psalter, as seems to be indicated by the arrangement of psalms in *4QPs^f*, but, again, definite decisions cannot be reached, due to the fragmentary state of the manuscript. (18) Anyhow, in Flint's opinion, *11QPs^a* was the foremost representative of the Psalter in Qumran, its qualification as a true scriptural Psalter being confirmed by its attribution to David, its structure and its usage. (19)

II. The Two Textual Witnesses of *Ps 151* and Their Interrelation

Not only did the deviating order of the psalms in *11QPs^a* and the additional compositions included in it bring forth diverging views on the fixation of the Psalter during the Second Temple Period; but the scroll has also aroused great excitement among scholars because it offered a Hebrew text parallel to *Ps 151*, the peculiar psalm 'outside the number' at the end of the LXX Psalter. Soon after the scroll had been unrolled in November 1961, it was realised by Sanders that the two final compositions of the scroll, of which the second has only been poorly preserved but was clearly separated from the first by a blank space of almost a line, comprised a longer version of the same autobiographical composition ascribed to David. In his opinion, these psalms '151A' and '151B' – as he termed them – represented the Hebrew original lying back of *Ps 151* LXX, which had been drastically amalgamated at the expense of the meaning and beauty of the original. (20) In his preliminary publication of the compositions, Sanders raised the possibility that these two psalms provided evidence of the depiction of David as Orpheus in the inter-testamental period, (21) but this suggestion was soon opposed by Rabinowitz,

(18) Flint, *The Dead Sea Psalms Scrolls*, 168-170.

(19) *Ibid.*, 222-226 and *Id.*, "The Book of Psalms," 468. According to S. Talmon, "The Crystallization of the 'Canon of Hebrew Scriptures' in the Light of Biblical Scrolls from Qumran," in *The Bible as Book. The Hebrew Bible and the Judaean Desert Discoveries*, ed. E.D. Herbert and E. Tov (The Bible as Book 4, London - New Castle - Grand Haven: British Library, 2002), 5-20, p. 18, n. 56, the scriptural nature of *11QPs^a* as argued by Flint does not invalidate the earlier proposition of Goshen-Gottstein and himself that the scroll served as a liturgical collection (see notes 4 and 5 above).

(20) See J.A. Sanders, "Ps. 151 in 11QPss," ZAW 75 (1963): 73-86, pp. 59-61 and *Id.*, *The Psalms Scroll of Qumrân Cave 11 (11QPs^a)* (DJD 4, Oxford: Clarendon, 1965), 59-61. Soon afterwards, his observation was confirmed by P.W. Skehan, "The Apocryphal Psalm 151," CBQ 25 (1963): 407-409, p. 407.

(21) Cf. Sanders, "Ps. 151 in 11QPss," 82: "A picture of mute animals and trees being charmed by music from the lyre comes to mind and with such a picture the myth of Orpheus."

who forcefully contended that the compositions did not exhibit any trace of Orphism whatsoever. (22) Although Sanders himself consequently expressed great caution about his earlier proposal of an Orphic connection in his full critical edition of the scroll, (23) it had by then already been made known to a more popular audience in the writings of A. Dupont-Sommer, who sought to prove that the psalm contained traces of Orphism and Pythagorism. (24) As for the shorter edition extant in the Septuagint, Dupont-Sommer was convinced that it resulted from a deliberate attempt to remove any Hellenistic tread from a work attributed to David. (25) Some years later, he was joined by the German scholar R. Meyer, who described *Ps* 151 LXX as a 'dogmatic correction' of a bucolic poem. (26) Another French scholar, J. Magne, equally subscribed to the Orphic interpretation of *Ps* 151A, though denying that it contained any trace of Pythagorism. (27) In one of his contributions published in the *Revue de Qumran*, Magne specified that the author of the original composition would have attached to David three features traditionally attributed to Orpheus: charming nature with his songs, inventing accompanying instruments and establishing a cult of praise. (28) At the same time, however, Magne questioned the idea that the translator would have consciously removed the allusions to Orpheus, as in his view the translator was unaware of any Orphic connection. (29)

Notwithstanding this more nuanced approach to the question of Orphism in *Ps* 151A, F.M. Cross shortly afterwards entirely rejected the alleged Orphic undertone. In a short but vigorous article, he ar-

(22) Cf. I. Rabinowitz, "The Alleged Orphism of 11Q Pss 28,3-12," *ZAW* 76 (1964): 193-200, p. 193: "The notion that Orphism is present in this psalm is one which, if allowed to become current, can only further bedevil the already speculation-haunted domain of Qumran-text scholarship."

(23) Cf. Sanders, *DJD* 4, 63: "No suggestion should be made on the basis of the supposed Orphic imagery in *Ps* 151 A that any facet of Orphism was consciously subscribed to by the writer of the poem or by his readers."

(24) See A. Dupont-Sommer, "Le Psaume cli dans 11QPs^a et le Problème de Son Origine Essénienne," *Semitica* 14 (1964) 25-62 and *Id.*, "Hébreu et Araméen," *Annuaire du Collège de France* 64 (1964): 309-324. Reference should also be made of his communication during the *séance publique annuelle de l'Institut de France* on 26 October 1964, entitled "David et Orphée".

(25) See particularly Dupont-Sommer, "Le Psaume cli," 56 and *Id.*, "Hébreu et Araméen," 319-320.

(26) R. Meyer, "Die Septuaginta-Fassung von Psalm 151,1-5 als Ergebnis einer dogmatischen Korrektur," in *Das ferne und nahe Wort*, ed. F. Maass (BZAW 105, Berlin: Töpelmann, 1967), 164-172, p. 172.

(27) Cf. J. Magne, "Les Textes Grec et Syriaque du Psaume 151," *RevQ* 8/32 (1975): 548-564, p. 557: "L'abréviateur n'a pu vouloir évidemment supprimer l'allusion au pythagorisme puisqu'elle n'existe [...] que dans l'imagination fertile et maximisante d'A. Dupont-Sommer." *Comp. Id.*, "Orphisme, Pythagorisme, Essénisme dans le Texte Hébreu de Psaume 151?," *RevQ* 8/32 (1975): 508-545, p. 545.

(28) Magne, "Orphisme, Pythagorisme, Essénisme," 543.

(29) See Magne, "Les Textes Grec et Syriaque," 560.

gued that Dupont-Sommer's elaboration of Sanders' proposal lacked any textual basis and that Magne's views were built on false readings. (30) Similarly, the Orphic interpretation has been dismissed by P. Skehan and by M. Smith. Though the former merely noticed that he was not convinced of an Orphic colouring of the language of *Ps* 151, (31) the latter took the view that the original poem described David as presenting his praises of God as burnt offerings, and that *11QPs^a* contained a mutilated form of the poem, in which a colon had been removed. (32) In more recent times, N. Fernández Marcos too discarded the suggestion that the presumed Orphic features of David caused the abbreviation of the psalm in the Septuagint. (33)

Be that as it may be, it is important to note with respect to the interrelation of *Ps* 151 LXX and *Ps* 151A that Sanders' outlook on the textual development of *Ps* 151 has met with virtually general approval for more than two decades. Not earlier than 1988, this view – which basically amounts to *Ps* 151 LXX being (a translation of) an amalgamation of *Ps* 151A-B – was seriously challenged by the linguistic analysis of M. Haran, on which Fernández Marcos heavily relied for his rejection of the Orphic interpretation. Pointing out that all the indications of Late Hebrew and all the artificial and forced expressions appeared in the 'extra' parts of the psalm, Haran arrived at the conclusion that the Hebrew text behind *Ps* 151 LXX reflects the original extent of the Psalm, *Ps* 151A-B being expanded and derivative. (34) Although Haran eventually sought to reaffirm that *11QPs^a* is simply a liturgical collection of psalms taken from an already fixed biblical Psalter, it cannot be denied that he strongly made his case that Sanders' intuitive suggestion lacked sound arguments. In point of fact, it was almost exclusively built on the assumption that *Ps* 151 LXX in itself makes little or no sense at all. (35) As Haran is, however, first and foremost concerned with the status of *11QPs^a*, the question presents itself whether his arguments are of a more ob-

(30) F.M. Cross, "David, Orpheus and Psalm 151:3-4," *BASOR* 231 (1978): 69-71.

(31) P.W. Skehan, "Again the Syriac Apocryphal Psalms," *CBQ* 38 (1976): 143-158, p. 147.

(32) M. Smith, "Psalm 151, David, Jesus and Orpheus," *ZAW* 91 (1981): 247-253.

(33) N. Fernández Marcos, "David the Adolescent: on Psalm 151," in *The Old Greek Psalter*, ed. R.J.V. Hiebert, C.E. Cox, and P.J. Gentry (JSOT SS 332, Sheffield: Academic Press, 2001), 205-217, pp. 209-210. However, E. Zenger, "David as Musician and Poet: Plotted and Painted," in *Biblical Studies/Cultural Studies. The Third Sheffield Colloquium*, ed. J.C. Exum and S.D. Moore (JSOT SS 266, Sheffield: Academic Press, 1998), 263-298, subscribes to the viewpoint of Dupont-Sommer *et al.* that *Psalm* 151A portrays David as the Jewish Orpheus and that the Greek translator rejected this interpretation of David as musician.

(34) M. Haran, "The Two Text-Forms of Psalm 151," *JJS* 39 (1988): 171-182, pp. 176-178.

(35) See Sanders, "Ps. 151 in 11QPss," 79-80 and *Id.*, *DJD* 4, 59-60.

jective nature, even if they have been endorsed by M. Smith and M. Segal. (36)

In view of these mutual exclusive proposals on the question of the interrelation of the two textual witnesses of *Ps* 151, it could be called into question whether the assumption of literal dependence in one or the other direction forms a good starting point. For this reason, I have most recently proposed in a paper presented to the *Colloquium Biblicum Lovaniense* to consider *Ps* 151 LXX and *Ps* 151A-B as ‘variant literary editions’ which are not necessarily directly related to each other. (37) After having scrutinised both versions of the psalm from a translation-technical perspective – thereby using the criteria outlined by Barr, Tov, Aejmelaeus and Ausloos-Lemmelijn (38) – I came to the conclusion that the Greek translator of *Ps* 151 LXX was generally faithful to his *Vorlage*. Though he occasionally departed from it by slightly adding a word or a semantic dimension, not a single omission has been found in those portions of the psalm evidenced in both textual witnesses. Since it would do injustice to such a translator to hold him responsible for the major divergences between *Ps* 151 LXX and *Ps* 151A-B, I explicitly rejected the oversimplifying statement in the *Dead Sea Scrolls Bible* of the translator having re-worked and synthesised two Hebrew psalms into a single Greek composition.

However, as a translation-technical investigation evidently cannot be decisive when it comes to the interrelation between the supposed *Vorlage* of *Ps* 151 LXX and its counterparts in *11QPs^a*, I also pointed out the necessity of further research on this topic from a different perspective. (39) Since, according to Ulrich, the isolation of a

(36) M.S. Smith, “How to Write a Poem: The Case of Psalm 151a (11QPs^a 28.3-12),” in *The Hebrew of the Dead Sea Scrolls and Ben Sira*, ed. T. Muraoka and J.F. Elwolde (STDJ 26, Leiden: Brill, 1997), 182-208, pp. 185-187 and M. Segal, “The Literary Development of Psalm 151: A New Look at the Septuagint Version,” *Textus* 21 (2002): 139-158, p. 154.

(37) See my contribution Debel, *Amalgamator* (forthcoming). Be it noted that, although Ulrich mainly focuses on ‘variant literary editions’ of biblical books, he also holds that, frequently, different literary editions of a pericope can be distinguished. See, e.g., his earlier quoted definition in “The Canonical Process,” 278 (note 12 above).

(38) For these criteria, see basically J. Barr, *The Typology of Literalism in Ancient Biblical Translations* (MSU XV, Göttingen: Vandenhoeck & Ruprecht, 1979); E. Tov, *The Text-Critical Use of the Septuagint in Biblical Research* (JBS 8, Jerusalem: Simor, 21997), 20-24; A. Aejmelaeus, “Characterizing Criteria for the Characterization of the Septuagint Translators: Experimenting on the Greek Psalter,” in *The Old Greek Psalter*, ed. R.J.V. Hiebert, C.E. Cox, and P.J. Gentry (JSOT SS 332, Sheffield: Academic Press, 2001) and H. Ausloos and B. Lemmelijn, “Content Related Criteria in Characterising the LXX Translation Technique,” in *Proceedings LXX Fachtagung Wuppertal 2008* (forthcoming). Further bibliographical notes can be found in Debel, *Amalgamator* (forthcoming).

(39) See the conclusion of *ibid.*

‘variant literary edition’ depends on the presence of a reasonably large number of textual variants exhibiting an intentional pattern, (40) my earlier claim would be substantiated if it could be demonstrated that such a large-scale pattern is displayed in one of the textual witnesses of *Ps* 151. As *Ps* 151A-B constitute the longer edition of the psalm, the Qumran version is probably the best candidate to look for a deliberate cluster of ‘pluses’. Yet, due to the material conditions of *11QPs^a*, the investigation will necessarily be confined to *Ps* 151A. Its Orphic interpretation having been dismissed in contemporary research, the next section of the present paper will explore new possibilities of establishing a cluster of variants in *Ps* 151A, in order to prove that *Ps* 151A-B and *Ps* 151 LXX truly make up ‘variant literary editions’ of the same composition.

III. A ‘Midrashic’ Outlook on the Interrelation of *Ps* 151A-B and *Ps* 151 LXX

Within the context of the search for a literary pattern in *Ps* 151A, it has always been a commonplace to say that the text displays a *midrash* on *1 Sam* 16-17. As early as the full critical edition of the scroll in the *DJD*-series, the idea has been put forward by Sanders, (41) who had already noted in his preliminary publication of *Ps* 151 that the two final compositions of *11QPs^a* are based on *1 Sam* 16-17. (42) However, the idea needs to be carefully reconsidered, as the term *midrash* is often uncritically applied to *Ps* 151. This has been painfully exemplified in the already mentioned contribution of Fernández Marcos, who describes *Ps* 151 LXX as a ‘midrashic paraphrase’. (43) In my view, a distinction ought to be made between a ‘paraphrase’ of *1 Sam* 16-17, which comprises a summary of the main events related in those chapters, and a ‘*midrash*’, which implies by definition interpretative expansions to the text. (44) A mere paraphrase is not yet a *midrash*. In a similar vein, the article of Smith

(40) For bibliographical references to Ulrich’s concept of ‘variant literary editions’, see note 13 above.

(41) Sanders, *DJD* 4, 56.

(42) Sanders, “Ps. 151 in 11QPss,” 81. The same observation had already been made with respect to *Ps* 151 LXX by H.B. Swete, *An Introduction to the Old Testament in Greek* (Cambridge: University Press, ²1902), 253.

(43) Fernández Marcos, “David the Adolescent,” 213.

(44) Cf. the definitions of *midrash* in “Midrash,” *The Jewish Encyclopedia* 8 (1904): 548-550, p. 548 (“an exegesis which, going more deeply than the mere literal sense, attempts to penetrate into the spirit of the Scriptures, to examine the text from all sides, and thereby to derive interpretations which are not immediately obvious”) and in E. Tov, *Textual Criticism of the Hebrew Bible. Second Revised Edition* (Minneapolis, MN: Fortress Press; Assen: Royal Van Gorcum, ²2001), 128 (“By definition such midrashic elements add a dimension to the plain meaning of Scripture”).

should be criticised too, as he sets out to examine in detail the borrowings from *I Sam* 15-18 (*sic*) in order to corroborate Sanders' view that *Ps* 151A is a *midrash* to these chapters. (45) The plain fact that material has been borrowed from the biblical accounts of the anointing of David and his battle with Goliath, as Smith points out, does not yet constitute the text as a *midrash* to them.

Notwithstanding these critical remarks concerning the idea that *Ps* 151 is a *midrash*, I am still convinced that the clue to the puzzle of the textual development of this psalm can be found exactly in its 'midrashic' connection to *I Sam* 16-17. However, whereas Smith confines the *midrash* to the outer sections of *Ps* 151A, I am inclined to exclude these sections from the *midrash*, because they merely make up the 'paraphrase' of the events told in *I Sam* 16-17. As becomes clear from Smith's analysis, the central section of the poem does not contain borrowings from these chapters and is phrased in the idiom of the poet's day. (46) Yet, contrary to Smith, I do not infer from these observations that the *midrash* ends in the central section of the poem, (47) but rather that it is particularly present there, where the retelling has come to an end. Remarkably, this central section also lacks a parallel in *Ps* 151 LXX, a fact that has been insufficiently taken into account by Smith, who merely notes that "lines 6-7 represent the author-redactor's single greatest departure from either biblical and extra-biblical models or the Hebrew *Vorlage* of his composition". (48)

Even more significantly, it should furthermore be noted that the two other parts of *Ps* 151A which have no parallel in *Ps* 151 LXX are neither part of the retelling of *I Sam* 16-17. While the affirmation that David has been made leader of his people and ruler over the sons of the covenant (lines 11-12) is totally absent from Smith's 'building blocks' of *I Sam* 15-18 for *Ps* 151A, (49) the elaboration of the outward appearance of David's brothers (lines 9-10) is only listed insofar as it also has a parallel in the Septuagint version, which simply describes the brothers as *καλοὶ καὶ μεγάλοι*. In other words, none of the 'pluses' in *Ps* 151A contains borrowings from *I Sam* 16-17. The above observations on the central section of the poem and on the distinction between 'paraphrase' and *midrash*, occasion me to conclude that *Ps* 151 LXX and its parallel lines in *Ps* 151A comprise the 'para-

(45) Smith, "How to Write a Poem," 184, 187.

(46) The latter observation had already been made by A. Hurvitz, "The Language and Date of Psalm 151 from Qumran," *Eretz Israel* 8 (1967): 82-87. As I did not have access to this article, I have taken its reference from Haran, "Two Text-Forms", who arrives at the same conclusion as Hurvitz (see esp. pp. 176-177 in this regard).

(47) Smith, "How to Write a Poem," 197-199.

(48) *Ibid.*, 199.

(49) *Ibid.*, 195.

phrase', while those sections from *Ps* 151A without a parallel constitute the *midrash* to *1 Sam* 16-17.

Moreover, this 'midrashic' connection to *1 Sam* 16-17 in the extra parts of *Ps* 151A enables us to discover a deliberate pattern of variants in the longer edition of the psalm. The central section of the poem, introduced by the opening phrase *אמרתי אני בנפשי* – which is reminiscent of Qohelet's introductory formula *אמרתי אני בלבי* (50) – exhibits David's piety in a song of praise to God. As has been noticed by D. Amara, the extent of David's piety offers an explanation why he has been given preference over his brothers. (51) Even if they are taller and more handsome than him, as is emphasised in the second major 'plus' of the *midrash*, God did not choose them, because David had a stronger inner orientation towards God. Likewise, David's dedication to God explains why he has eventually been made ruler over his people, a fact to which attention is drawn in the third and final 'plus' of *Ps* 151A. As such, the three parts of the *midrash* are all related to David's piety, and hence to each other; a pattern of variants has been isolated in *Ps* 151A. As a consequence, the suspicion that it represents a 'variant literary edition' has been all the more confirmed.

However, I part company with Amara when she maintains that such an explanation for the early selection of David is not offered in the account of his anointing in *1 Sam* 16. (52) In point of fact, it is quickly observed in *1 Sam* 16,7 that "man looks at the outward appearance, but יהוה looks at the heart". Therefore, in my view, the set of textual variants in *Ps* 151A is a *midrash* to *1 Sam* 16,7, elaborating on what יהוה saw in the heart of David. This *midrash* has been introduced in the 'paraphrase' of *1 Sam* 16-17 as a whole, which retells David's removal from behind his sheep and his victory over Goliath. (53) While the paraphrase is restricted to a summary of the events, the three parts of the *midrash* emphasise that, though David's brothers were handsome and tall, he alone gave praise to God in his heart, which is why he has been given the kingship by God. The elevation of David is taken to exemplify that God does not judge people according to their outward appearance, but according to their inner piety. In this way, the biblical tradition of David's anointing was enriched and made relevant to the contemporary situation by a creative scribe, or as Ulrich puts it: the tradition, which was important in itself and in its original setting, was found also to be important to the here and now. (54)

(50) This analogy has already been observed by Sanders, *DJD* 4, 57.

(51) D. Amara, "Psalm 151 from Qumran and Its Relation to Psalm 151 LXX. English Abstract," *Textus* 19 (1998): 183-185, p. 183.

(52) *Ibid.*, 183.

(53) This midrashic connection has already been noted by Sanders, *The Dead Sea Psalms Scroll*, 95, but he failed to observe that *1 Sam* 16,7 accounts for all the 'pluses' in *Ps* 151A.

(54) See Ulrich, "The Bible in the Making," 84. Sanders refers to this aspect as

In this line of reasoning, *Ps* 151 LXX and its alleged *Vorlage* represent a piece of art on its own account, which is by no means meaningless, as Sanders once contended. (55) The only inference that can be drawn from the 'pluses' in *Ps* 151A, is that both versions of *Ps* 151 clearly have a different purpose. When it is accepted that *Ps* 151 LXX and its *Vorlage* merely intend to retell the story of *1 Sam* 16-17, there is no reason why this psalm should be considered meaningless. Therefore, it is undoubtedly more appropriate to term *Ps* 151 LXX 'a paraphrase of *1 Sam* 16-17 in the style of an autobiographical ode', (56) the *Vorlage* of which has been enlarged with a *midrash* on *1 Sam* 16,7 in *Ps* 151A. As such, both textual witnesses provide excellent examples of the lively interest in the books of Samuel during the late biblical period. (57)

IV. The Textual History of *Ps* 151

The above 'midrashic' outlook on the interrelation of *Ps* 151 LXX and *Ps* 151A-B has made clear that, on the one hand, both *Ps* 151 LXX and *Ps* 151A-B contain the same paraphrase of the events recounted in *1 Sam* 16-17, and that, on the other hand, a threefold addition to this paraphrase in *Ps* 151A has enriched it with a *midrash* on *1 Sam* 16,7. As a consequence, the Septuagint version is likely to stand closer to the original text, thus representing the 'more primitive edition', (58) at least if it is accepted, as both Tov and Ulrich main-

'resignification', see J.A. Sanders, *Canon and Community. A Guide to Canonical Criticism* (Guides to Biblical Scholarship, Old Testament Series, Philadelphia, PA: Fortress, 1984), 22.

(55) See note 35 above.

(56) The terminology has been taken over from Talmon, but he uses it in his description of *Ps* 151 as such. See Talmon, "Pisqah Be'emsu' Pasuq," 16 and *Id.*, "Extra-Canonical Hebrew Psalms from Qumran - Psalm 151," in *The World of Qumran from Within. Collected Studies*, ed. S. Talmon (Leiden: Brill, 1989), 244-272, p. 270.

(57) Concerning this interest, see A. Van der Kooij, "The Story of David and Goliath. The Early History of Its Text," *ETL* 68 (1992): 118-131, p. 130 and Fernández Marcos, "David the Adolescent," 216. In this regard, another illustration of the interest in the books of Samuel can be found in the variant editions of the account of the battle of David with Goliath in *1 Sam* 17-18 of LXX and MT. With respect to these editions, reference should at least be made of the groundbreaking *joint venture* of D. Barthélemy, D. Gooding, J. Lust and E. Tov, which has been published as *The Story of David and Goliath. Textual and Literary Criticism. Papers of a Joint Research Venture* (OBO 73, Fribourg: Editions Universitaires; Göttingen: Vandenhoeck & Ruprecht, 1986). Concerning *1 Sam* 17-18, see also, e.g., A. Rofé, "The Battle of David and Goliath: Folklore, Theology, Eschatology," in *Judaic Perspectives on Ancient Israel*, ed. J. Neusner, B.A. Levine, and E.S. Frerichs (Philadelphia, PA: Fortress, 1987), 117-151, pp. 119-123; J. Trebelle, "The Story of David and Goliath (1 Sam 17-18): Textual Variants and Literary Composition," *BIOSCS* 23 (1990): 16-30, pp. 27, 29 and Ulrich, "The Dead Sea Scrolls and the Biblical Text," 90.

(58) This terminology has been taken from A. Aejmelaeus, "Septuagintal

tain, that the general tendency of the scribes was to expand the scriptural text, rather than to remove anything from it. (59) Furthermore, there is no obvious explanation why the three 'pluses' in *Ps* 151A would have been suppressed, as the alleged Orphic colouring of these portions of the poem has not proven to be convincing, while, on the contrary, the 'midrashic' connection to *1 Sam* 16,7 accounts for their addition. Taking into consideration my own translation-technical examination of the two versions, which indicated that *Ps* 151 LXX is a faithful translation of a Hebrew source text that definitely does not correspond to *Ps* 151A-B, (60) the following picture of the textual history of *Ps* 151 emerges.

At the origins of both the extant versions of *Ps* 151 most likely stands a now lost Hebrew composition in which David autobiographically relates his anointing and his fight with Goliath. As it cannot be ruled out that this Hebrew original on which both *Ps* 151 LXX and *Ps* 151A-B are dependent was a revised edition of yet another earlier composition, (61) it is probably best to term it 'edition *n*', '*n*' standing for the unknown number of previous editions. (62) At a certain moment in time, 'edition *n*' has been faithfully translated into Greek and added as a supernumerary psalm to the Greek Psalter, which subsequently caused its inclusion in the Syriac Psalter along with four other psalms from outside the Masoretic Psalter. (63) Independently, 'edition *n*' was revised by a creative scribe, who expanded the 'paraphrase' of *1 Sam* 16-17 with a *midrash* on *1 Sam* 16,7, albeit without removing any of the events referred to in his source text. At the same time, this scribe separated the Goliath episode from the calling and anointment of David, and may have enriched it with 'midrashic' elements, too, but the damaged state of *11QPs^a* at this juncture does not allow drawing conclusions in this regard.

Translation Techniques - a Solution to the Problem of the Tabernacle Account," in *Septuagint, Scrolls and Cognate Writings. Papers Presented to the International Symposium on the Septuagint and Its Relations to the Dead Sea Scrolls and Other Writings* (Manchester, 1990), ed. G.J. Brooke and B. Lindars (SBL SCS 33, Atlanta, GA: Scholars Press, 1992), 381-401, p. 395.

(59) See, e.g., E. Tov, "The Nature of the Differences between MT and the LXX in 1 Sam. 17-18," in *The Story of David and Goliath*, 19-46, pp. 38-39 and Ulrich, "Multiple Literary Editions," 98.

(60) Debel, *Amalgamator* (forthcoming).

(61) As this cannot be determined from the evidence at hand, this phase in the textual development of *Ps* 151 belongs to the 'prehistory' of the text. For this terminology, see B. Lemmelijn, "What Are We Looking for in Doing Text-Critical Research?," *JNSL* 23/2 (1997): 69-80, p. 77.

(62) See Ulrich, "Pluriformity," 39; *Id.*, "The Dead Sea Scrolls and the Biblical Text," 85 and *Id.*, "The Dead Sea Scrolls and the Hebrew Scriptural Texts," 128.

(63) Following the lead of Dupont-Sommer, "Le Psaume cli," 50 and J. Strugnell, "Notes on the Text and Transmission of the Apocryphal Psalms 151, 154 (= Syr. II) and 155 (= Syr. III)," *HTR* 59 (1966): 258-281, p. 265, who both argue that *Ps* 151 Syr is a daughter version of *Ps* 151 LXX. Pace Magne, "Les Textes Grec et Syriaque," 563.

As both *Ps* 151 LXX and *Ps* 151A-B developed autonomously from 'edition *n*', they can be designated respectively 'edition (*n*+1)^a' and 'edition (*n*+1)^b'. Though 'edition (*n*+1)^b' is, due to its expansionistic elements, 'more developed' (64) than 'edition (*n*+1)^a', it is not *mutatis mutandis* the 'later' edition, as translation and expansion came into being independently from each other. At most, the Septuagint version could be called the 'earlier' edition from a literary perspective, which does not necessarily correspond to a historical point of view. (65) As such, the superscriptions 'a' and 'b' merely refer to the sequence in the literary development of the psalm and do not indicate any chronological development. The evidence at hand is simply too limited to make inferences with respect to the relative chronology of both editions.

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(64) See note 58 above.

(65) For this distinction, cf. Tov, "The Nature of the Differences," 42.

A SCROLL IN ONE HAND AND A MATTOCK IN THE OTHER:

Latrines, Essenes, and Khirbet Qumran

Summary

In the fall of 2006, dozens of humorously titled articles, such as “Biblical Latrine: Ancient Parasites Show That Cleanliness May Have Been Next To Sickliness” and “How Toilet Habits Killed off Dead Sea Scrolls Sect,” appeared in The New York Times, The Independent, MSNBC and a variety of other mainstream publications around the world. These non-academic articles detailed the discovery of a purportedly ancient latrine to the northwest of Qumran, which, according to a recent article in *Revue de Qumran* by Joe Zias, James Tabor, and Stephanie Harter-Lailheugue proves that the individuals who lived at the site of Qumran during the Second Temple period were none other than the Jewish sect known as the Essenes. Based upon certain bioarchaeological evidence from the Qumran plateau, the witness of Josephus, who describes the defecation habits of the Essenes (*Bellum* 2.147-49), and a passage from the *Temple Scroll*, which calls for latrines to be built 3,000 cubits to the northwest of the “city of the Temple” (*11Q19* 46.13-16; cf. *Deut* 23:12-14), Zias *et al* claim that the authors of the Dead Sea Scrolls understood their community to be a physical replacement for the Temple (cf. *4Q174* 1 2 i 6; *1QS* 9.4-6) and, as such, they attempted to protect the “city of the Temple” (i.e., Qumran) from the impurity of excrement by building communal latrines to the northwest of their habitation. In contrast to Zias *et al*’s approach, which is founded upon a systematic reading of the Greek and Qumran sources, the following paper will attempt to establish the Dead Sea Scroll’s position on the subject of excrement by focusing on the archaeological and literary evidence from Qumran (*1QM* 7.6b-7; *4Q265* 6 2; *4Q472a* 2-4; and *11Q19* 46.13-16). In the end, it will be argued that the evidence at our disposal is inconsistent and that it cannot be used to recreate a cohesive and consistent approach on the subject of defecation at Qumran.

IN THE fall of 2006 dozens of mainstream media outlets began reporting on the discovery of a purportedly ancient latrine to the northwest of Qumran, which, according to an article in *Revue de Qumran* by Joe Zias, James Tabor and Stephanie Harter-Lailheugue, (1) proves that the individuals who lived at the site of Qumran during the Second Temple period were none other than the Jewish sect known as the Essenes. Based upon the witness of the Dead Sea Scrolls, the writings of Josephus and newly acquired parasitological evidence from the Qumran plateau, Zias *et al* conclude: "This important new evidence bolsters the Essene hypothesis by corroborating the descriptions of this distinctive toilet regimen in both the Scrolls and Josephus." (2)

By the spring of 2007, Zias *et al*'s [hereafter Zias] theory had, in the eyes of many non-specialists, become an indisputable fact. Take, for example, the following quote from *Archaeology* magazine:

An ancient latrine near the ruins of Qumran follows the unusual and stringent guidelines in both the Dead Sea Scrolls and historical accounts of the strict Jewish Essene sect – directly linking the sect, the scrolls, and the settlement as never before. The latrine was required to be hidden a specific distance northwest of the city, but it may have been unsanitary, thus contributing to the poor health of Qumran's ancient residents. (3)

Although the Qumran/Essene hypothesis is, by far and away, the most convincing explanation for the textual and archaeological evidence thus far recovered from the region in and around Khirbet Qumran, one of the great drawbacks of this theory is that it has so completely dominated the landscape of Dead Sea Scrolls research that we are frequently unable to see beyond the shadow that it casts. This is particularly true in regards to the work of Zias who, rather than allowing the archaeological and literary evidence to speak for itself, have attempted to validate the Qumran/Essene hypothesis by actively searching the Qumran plateau for evidence that supports the witness of Josephus and the Scrolls. This criticism is confirmed by the authors themselves when, in describing the rationale behind their study, they note:

In the summer of 1996 Dr. Tabor and Joe Zias performed a walking survey of the northwest area [of the Qumran plateau], based on the descriptions in both the Scrolls and Josephus. Dr. Tabor pointed out several places at which more permanent [toilet] facilities may have been

(1) Joe E. Zias, James D. Tabor, and Stephanie Harter-Laiheugue, "Toilets at Qumran, the Essenes, and the Scrolls: New Anthropological Data and Old Theories," *RevQ* 22/4 (2006): 631-40.

(2) Zias, Tabor, and Harter-Laiheugue, "Toilets at Qumran," 631.

(3) "World Roundup," *Archaeology* 60/2 (2007): 10-11.

present, but the random sampling indicated that the entire northwest area, regardless of any remaining facilities, appears to have been used by the community for defecation. (4)

Beyond the observation that the authors have, to date, not presented any artefacts or definitive archaeological evidence to support their claims, one is struck by the overwhelmingly subjective nature of their approach. Not only have their preconceived notions about the Scrolls and the Qumran/Essene hypothesis affected the way in which they have interpreted the material culture and geographical features of the Qumran plateau, but their interpretation of the geographical features and material culture has, in turn, been used to verify the Qumran/Essene hypothesis and the witness of the scrolls. (5) This form of circular argumentation is both unproductive and misleading. In the following discussion we will challenge Zias' theory by examining the relevant literary evidence and the archaeological data from the Qumran plateau.

Texts and Toilets

According to Zias, "the Scrolls tell us that the sect was forbidden to go to the toilet within the city walls of Jerusalem." (6) In point of fact, the prohibition against defecating in the city of Jerusalem exists *solely* in the *Temple Scroll*:

(11Q19 46.13-16a)

ועשיתה להמה מקום יד חוץ מן העיר אשר יהיו יוצאים שמה	13
לחוץ לצפון המערב לעיר בתים ומקורים ובורות בתוכמה	14
אשר תהיה הצואה יורדת אל תוכמה ולי"א תהיה נראה לכול רחוק	15
vacat מן העיר שלושת אלפים אמה	16a

13	And you will make for them a place of the hand outside of the city where they shall go;
14	outside to the northwest of the city - houses with beams and pits in their midst
15	into which excrement shall drop and shall not be visible to anyone at a distance
16a	from the city of three thousand cubits vacat

(4) Zias, Tabor, and Harter-Laiheugue, "Toilets at Qumran," 634n.10.

(5) "[A]rchaeology," as Phillip Davies has noted, "needs to work as far as possible without certain preconceptions. In particular, it should never set out to prove a previously held theory, for my impression is that one can make archaeology prove so many things. Preconceptions lead to overinterpretation, which is much worse than underinterpretation. When observation and theory become mixed up in the process of describing a site, the uninformed reader will likely be misled." Phillip Davies, "How Not to Do Archaeology: The Story of Qumran," *BA* 51/4 (1998): 206.

(6) Zias, Tabor, and Harter-Laiheugue, "Toilets at Qumran," 632.

What's more, in contrast to the rather emphatic interpretation offered by Zias, who states that the members of the sect were "forbidden to go to the toilet within the city walls of Jerusalem," the Temple Scroll's prohibition on defecation is not explicit. Rather, the author/redactor of this document implicitly prohibits individuals from relieving themselves in Jerusalem through his call to construct permanent latrines 3,000 cubits to the northwest of the city. Indeed, the *Temple Scroll* never specifies that only those who were members of the "sect" were prohibited from relieving themselves in the city of Jerusalem. On the contrary, this legislation, as well as the remainder of the Temple Scroll's rulings, appears to have been intended for everyone within the city, regardless of which group they might have been affiliated with. Zias' reading of the *Temple Scroll* ignores both the question of the document's authorship and its seemingly utopian genre. If, as many have argued, the *Temple Scroll* is a protosectarian document that describes a state of affairs at odds with the world in which it was written, then one wonders how these factors might impact Zias' interpretation of the document and his subsequent use of the Temple Scroll to prove the existence of a latrine to the northwest of Khirbet Qumran.

In furthering the argument that the Qumran community built latrines to the northwest of Khirbet Qumran, Zias also appeals to the *War Scroll*:

(IQM 7.6b-7)

		ורוח יהיה	6b
		בין כול מחניהמה למקום היד כאלפים באמה וכול ערות	7
		דבר רע לוא יראה סביבות כול מחניהם	
6b	And there shall be a distance		
7	between all of their camps and the place of the hand two thousand cubits. And any immodest nakedness shall not be seen around any of their camps.		

According to Zias, "the *War Scroll* (7:6-7) specifies that the distance between the latrines (literally, "place of the hand") and the camp should be two thousand cubits." (7) Although this is an accurate reading of the passage above, Zias misrepresents the *War Scroll* by claiming that, "the *Temple Scroll* and the *War Scroll* speak of permanent roofed facilities." (8) As is clear in the passage quoted above, the *War Scroll* says no such thing about a roofed structure. Given that the war camp would have been mobile rather than stationary, it stands to reason that the latrines envisioned by the author/redactor of the *War Scroll* would have been temporary rather than permanent.

(7) Zias, Tabor, and Harter-Laiheugue, "Toilets at Qumran," 632.

(8) Zias, Tabor, and Harter-Laiheugue, "Toilets at Qumran," 633n.7.

Beyond the issue of permanent versus temporary latrines, the most notable differences between the *Temple Scroll* and the *War Scroll*'s rulings on latrines involve the lack of agreement concerning their location (i.e., to the northwest of Jerusalem or outside the war camp) and the distance between the latrines and the city or camp (i.e., 3,000 cubits or 2,000 cubits). Regarding the issue of location, it is interesting to note that the author/redactor of the *Temple Scroll* frequently applies the concept of the war camp to Jerusalem, which suggests that a similar ideological framework has been embraced by the authors of both documents (i.e., that God was thought to be present in both the war camp and the city of Jerusalem and that God should not be exposed to any kind of indecency that might cause him to turn away from his people; cf. *Deut* 23:13-15).

As for the difference in distance between the latrines and the city or camp, it is difficult to determine why the *Temple Scroll* demands a greater distance than the *War Scroll*. While it is possible that the *Temple Scroll*'s ruling may have been based on a detailed understanding of Jerusalem's elevation and topography, (i.e., information that may have necessitated a greater distance between the city and the latrines so as to prevent God or anyone else from "seeing" anything indecent), it is equally possible that the distances are different due to the uniqueness of the situation in each document. As Jodi Magness has suggested: "[the *Temple Scroll*] and the *War Scroll* added a distance regulation because of the state of purity required in the ideal holy city or during the war at the end of days." (9)

For Zias, the distances between the latrines and the city or camp in the *Temple Scroll* and *War Scroll* are relevant in that they (1) support his notion that there was a latrine beyond the walls of Khirbet Qumran and (2) they verify Josephus' claim that the Essenes refrained from defecating on the Sabbath. (10) Concerning the latter, Zias cites a Sabbath regulation in the *Damascus Document* that prohibits individuals from walking more than 1,000 cubits outside of their city of residence (CD 10.21). This ruling, argues Zias, supports the notion that Josephus and the Scrolls are in agreement on the issue of relieving oneself on the Sabbath. According to Zias:

If the latrines were placed at a distance of 2,000 to 3,000 cubits, then on the Sabbath day a community member, in order to reach the latrine,

(9) Jodi Magness, *Archaeology of Qumran and the Dead Sea Scrolls* (Grand Rapids: Eerdmans, 2004), 109.

(10) "[On the Sabbath the Essenes do not] even go to the stool. On other days they dig a trench a foot deep with a mattock – such is the nature of the hatchet which they present to neophytes – and wrapping their mantle about them, that they may not offend the rays of the deity, sit above it. They then replace the excavated soil in the trench. For this purpose they select the more retired spots. And though this discharge of the excrements is a natural function, they make it a rule to wash themselves after it, as if defiled" (*War* 2.147-149).

would be required to walk farther than the permissible distance, thereby transgressing Sabbath law. (11)

The problem with this interpretation is that it assumes the rulings in the *Temple Scroll* and the *War Scroll* were applied directly to Khirbet Qumran. Beyond the observation that the geography of the Qumran plateau does not allow for the building of latrines 2,000 to 3,000 cubits to the northwest of Khirbet Qumran, (12) there is nothing in either the *Temple Scroll* or the *War Scroll* to indicate that their toilet legislations should be applied to anything other than the idealized city of Jerusalem or to the war camp at the end of days. If, as some have argued, the Qumran community eventually came to see themselves as replacing the Temple in Jerusalem, then it is theoretically possible that the community may have tried to apply the rules of the *Temple Scroll* to themselves. However, if this is the argument that Zias has in mind when he assumes that the scrolls' latrine regulations were binding at Qumran, he fails to provide any evidence to support the position.

Although Zias attempts to sidestep the "variations in practice" exhibited by the *Temple Scroll* and the *War Scroll* by claiming that he is more interested in focussing on "Josephus' reports about the Essenes," his conclusion that the "parasitological evidence regarding Qumran toilet practices ...clearly confirm the textual evidence in the Scrolls and Josephus for an Essene identification" (13) would seem to suggest otherwise.

Yet another difficulty with Zias' argument is the presence of what appears to be a toilet within the complex of buildings at Qumran. Although Yizhar Hirschfeld has argued against the installation in Locus 51 being identified as a toilet, (14) both Roland de Vaux (15) and Jodi Magness (16) have interpreted L.51 as being rep-

(11) Zias, Tabor, and Harter-Laiheugue, "Toilets at Qumran," 632.

(12) "Yet at Qumran," notes Zias, "due to geological constraints such as cliffs and steep ravines, it would be impossible to apply this rule of distance." Zias, Tabor, and Harter-Laiheugue, "Toilets at Qumran," 634.

(13) Zias, Tabor, and Harter-Laiheugue, "Toilets at Qumran," 639.

(14) According to Hirschfeld, L.51 cannot be a toilet as it is a "fixed installation lacking the water channels needed for flushing." Hirschfeld continues: "It seems unlikely that the occupants of the main building would have tolerated the nuisance caused by the location of such a toilet within the structure." Yizhar Hirschfeld, *Qumran in Context: Reassessing the Archaeological Evidence* (Peabody: Hendrickson, 2004), 100.

(15) Humbert, J.B., and A. Chambon, *Fouilles de Khirbet Qumran et de Ain Feshkha*. (Gottingen: Vandenhoeck and Ruprecht, 1994), 309.

(16) As Magness has observed: "De Vaux identified one of the installations he excavated as a toilet. This installation was located in L.51, a large room on the eastern side of the main building to the north of the miqveh in L48-49. A terracotta pipe set into a conical, mud-lined pit that was filled with thin layers of coarse dirty earth was embedded in the floor of this room. Comparisons with toilets in the ancient Mediterra-

representative of a toilet. Surprisingly, Zias has also embraced the idea that L.51 is a toilet and, in an article written in 2004, Zias, Stephanie Harter, Francoise Bouchet, and Kosta Mumchuglu claim to have verified L.51's status as a toilet through the examination of soil samples containing the eggs from a parasite that is only "excreted from the [human] body during defecation." (17) In an effort to deal with this seemingly contradictory evidence, Zias claims that the toilet in L.51 was only used for "faecal emergencies." (18) The difficulty with this interpretation, beyond the fact that it is an *ad hoc* argument that can neither be confirmed nor denied, is that it contradicts the very evidence that Zias uses to support the notion that latrines were built beyond the walls of Qumran. Specifically, if the rationale for building latrines 3,000 cubits outside of Jerusalem in the *Temple Scroll* and 2,000 cubits outside of war camp in the *War Scroll* was to prevent God from being exposed to any kind of indecency that might cause him to turn away from his people (cf. Deut 23:13-15), then the presence of a toilet within the walls of the Khirbet Qumran, regardless of whether it was used for "faecal emergencies" or not, would have violated this concept. In short, the presence of a toilet inside the walls of Khirbet Qumran contradicts both the witness of the scrolls and Josephus. (19)

Returning once again to Josephus' statement that the Essenes refrained from relieving themselves on the Sabbath, Magness notes: "If this regulation was observed at Qumran, the inhabitants presumably refrained from using the toilet in L.51 on the Sabbath." (20) Although displaying far more caution than Zias, Magness' hypothesis would appear to be similar to Zias' argument that the toilet in L.51 was only used for "faecal emergencies" in that it can neither be confirmed nor denied. In response to Magness' hypothesis, Albert Baumgarten notes:

If there was a latrine at Qumran, its implications for the identity of the group should not be averted by the intellectual acrobatics of the sort attempted by Magness ... there is no reason to think that defecation in this toilet was prohibited on the Sabbath. Its use [in contrast to the

nean world suggest that de Vaux's identification is correct." Magness, *Archaeology of Qumran and the Dead Sea Scrolls*, 105-06; idem, "Two Notes on the Archaeology of Qumran," *BASOR* 312 (1998): 37-40.

(17) Stephanie Harter, Francoise Bouchet, Kosta Mumchuglu, and Joe Zias, "Toilet Practices Among Members of the Dead Sea Scrolls Sect at Qumran (100 BCE – 68 CE)," *RevQ* 84 (2004): 579-85.

(18) Zias, Tabor, and Harter-Laiheugue, "Toilets at Qumran," 633-34.

(19) According to Magness: "The location of the toilet in L.51 on the eastern side of the main building suggests that the distance regulations mandated for the toilets in the War Scroll and in the Temple Scroll did not apply to the settlement at Qumran." Magness, *Archaeology of Qumran and the Dead Sea Scrolls*, 109.

(20) Magness, *Archaeology of Qumran and the Dead Sea Scrolls*, 110.

witness of Josephus and the scrolls] required neither carrying a shovel, nor digging a hole, nor walking beyond the limit permitted on the Sabbath. (21)

While the presence of a toilet in L.51 may well have a bearing on the issue of the Qumran community's identity, Baumgarten's comments regarding the rationale for prohibiting the act of defecation on the Sabbath are more directly related to the topic at hand. In particular, the observation that the toilet in L.51 did not require an individual to carry a tool, dig, or walk beyond the Sabbath limits suggests that the Sabbath regulations would not have applied to those wishing to relieve themselves in this installation. Once again we are confronted by the fact that the archaeological evidence from Qumran appears to contradict the witness of Josephus and the scrolls.

In an effort to clarify her hypothesis concerning the relationship between the toilet in L.51, the witness of the Dead Sea Scrolls and Josephus' description of the Essenes, Magness offers the following synthesis:

Because the toilet in L.51 could not have served the needs of the entire community, most of the members must have relieved themselves outside the settlement, either in built facilities or in the manner described by Josephus. If built facilities existed outside the settlement, they may have been located to the northwest. (22)

Not unlike Zias, Magness has attempted to create an all-encompassing theory in order to account for both the archaeological and textual evidence at our disposal. In truth, it is not easy to distinguish between Magness' hypothesis and that of Zias. For example, both agree that (1) there was a toilet in L.51, (2) that the majority of the Qumran community would have defecated outside of Khirbet Qumran in semi-permanent facilities or in accordance with Josephus' descriptions, and (3) that the communal latrines, if built, would have been situated to the northwest of settlement. Moreover, both Zias and Magness have attempted to account for all of the relevant information in such a way so as to make it fit within the confines of the Qumran/Essene hypothesis. (23)

(21) Albert Baumgarten, "Who Cares and Why Does it Matter? Qumran and the Essenes, Once Again!" *DSD* 11/2 (2004): 186.

(22) Magness, *Archaeology of Qumran and the Dead Sea Scrolls*, 113.

(23) Although there are many similarities between the hypotheses of Magness and Zias, it should be noted that Magness has been quite critical of Zias. In particular, Magness has rightly criticized Zias' appeal to a non-existent scholarly consensus on the issue of Essene fasting, which would have theoretically prevented the sectarians from having to use the toilet facilities on the Sabbath. Magness has also taken issue with Zias and his overly specific approach to the Greek sources that understands Josephus' descriptions of the Essenes as referring to the community living at Qumran. Jodi Magness, "Toilet Practices at Qumran: A Response," *RevQ* 22/2 (2005): 277-

Beyond the criticisms mentioned above (such as the fact that the presence of a toilet in the settlement of Qumran would have gone against both the *Temple Scroll* and the *War Scroll*'s stance on not offending God with any of the indecencies that accompany the act of defecation; cf. *Deut* 23:13-15), one notes an unwillingness on the part of Zias and Magness to acknowledge that the archaeological and textual evidence is problematic and difficult to reconcile. These difficulties are actually compounded by Magness who argues that "because the sectarians viewed themselves as a replacement for the temple and created by means of the sect a substitute for the sacrificial cult, temple purity laws were transferred to the lives of the members." (24) Although a valid argument in its own right, one wonders how it would have been possible for a group that considered itself the physical replacement for the Temple to tolerate a toilet in its midst (i.e., an installation that the *Temple Scroll* explicitly prohibits within the "city of the temple"; cf. *11QT* 46.13-16).

Before offering a theory of our own to account for the evidence discussed above, one final observation regarding Magness' hypothesis is in order. In an attempt to strengthen the relationship between the Qumran community, the Dead Sea Scrolls and the witness of Josephus, Magness enlists the witness of a document known as *4QHalakha C*:

(4Q472a 1-5)

] °°°° [1
]°°°[למכסִי צו אם לואִן	2
]ולִ שְׁלִישׁ עֶלְ פִי נשׁ[3
]לִ עַל מַעַשׁ[ה	4
]לִ[5

1]	[
2]	to cover excrements. If he does not[
3]	a vessel according to [
4]	regarding a dee[d
5]	[

According to Magness, "[this] fragmentary but significant halakhic (legal) scroll from Cave 4 at Qumran mentions the same practice of covering excrement that Josephus singled out for description." (25) Magness continues:

78. See also, Zias *et al*, "Toilet Practices Among Members of the Dead Sea Scrolls Sect at Qumran (100 BCE – 68 CE)," 579-85; Joe Zias, "Qumran Toilet Practices, A Response to a Response," *RevQ* 22/3 (2006): 479-84.

(24) Magness, *Archaeology of Qumran and the Dead Sea Scrolls*, 113.

(25) Magness, *Archaeology of Qumran and the Dead Sea Scrolls*, 110. Cf. Hannah Harrington, *The Purity Texts* (London: T&T Clark, 2004), 106-107.

The reference in line 2 to covering excrement recalls Josephus' Essenes, who dig a trench in the ground and, after defecating, 'they then replace the excavated soil.' Similarly, the Temple Scroll makes a point of mandating that the toilets be pits 'into which excrement will descend.' In other words, all of these sources – Josephus, the Temple Scroll, and 4Q472 – document or legislate the unique sectarian concern that excrement be concealed by being buried in a pit. (26)

The responsibility for transcribing and translating 4Q472a was given to Torleif Elgvin, who published his findings in *DJD* 35 in 1999. (27) Some six years after its publication, Elgvin and I had the opportunity to re-examine 4Q472a under the infrared camera in the IAA's scollery. After much discussion we determined that the readings of 4Q472a in *DJD* 35 were in need of correction and, at our request, new infrared images of this document were requested and obtained from the IAA. Our new readings of 4Q472a, which were published in the winter of 2007 in *Revue de Qumran*, (28) are reproduced here:

	וְרָצָה עֹרְרִי] ...[1
	יִאֲסֹף שְׁבֹטֵי נֹעַם לְטַעַם תָּם ב	2
	כָּל מִלֹּחַשׁ עֲרֹסְתּוֹ נָב]	3
	יִלְּךָ] אֶל כָּל עַמִּי	4
	יִלְּךָ]	5
1] ... and hasten, awake(?) [
2	He]will gather (the) tribes of (his) delight, to pla[nt them in	
3]all his kneaded dough will be ea[ten(?)	
4] ... therefore his peo[ple	
5] [

As the transcription and translation above suggests, 4Q472a has nothing to do with excrement or ritual purity. Although not entirely clear, the new reading of 4Q472a would seem to suggest that this fragment is representative of an eschatological work of salvation. Based upon this new evidence, Elgvin and I have argued that this text should be removed from the category of *halakhic* texts and re-assigned to the category of parabiblical material. It goes without saying that our new reading greatly affects Magness' observations regarding 4Q472a and severely limits the amount of weight that can be placed on the notion that the scrolls are in agreement with Josephus on the issue of defecation. (29)

(26) Magness, *Archaeology of Qumran and the Dead Sea Scrolls*, 110.

(27) Torleif Elgvin, "4QHalakha C," in *Qumra Cave 4 XXV Halakhic Texts* (*DJD* 35; Oxford: Clarendon Press, 1999), 155-56.

(28) Torleif Elgvin and Ian Werrett, "4Q472a in Infrared Light: Latrine Manual Down the Drain." *RevQ* 23/2 (2007): 261-268.

(29) In a private communication with Magness it was brought to my attention

Even the most committed supporter of the Qumran/Essene hypothesis must admit that the presence of a toilet in the site of Qumran presents a significant challenge. Although de Vaux claims to have found evidence that the toilet in L.51 was protected from the elements by a wooden roof (30) - a fact that would appear to parallel the description of the latrines in the *Temple Scroll* - it is important to note that none of L.51's remaining features agree with the relevant literary material, such as the location and the distance regulations mentioned above.

One possible way around this impasse involves a piece of evidence yet to be discussed. According to de Vaux, the toilet in L.51 and the adjoining *miqveh* in L.48-49 were destroyed by a massive earthquake in the year 31 BCE. (31) After this disaster, much of the eastern portion of the site, including both the toilet in L.51 and the *miqveh* in L.48-49, were left in ruins and never rebuilt. (32) De Vaux failed to find any definitive archaeological evidence of a replacement latrine being built within the site from 31 BCE until the Romans destroyed the site in 68 CE. Based on this evidence, or lack thereof, it would appear that the site Khirbet Qumran did not contain a toilet for nearly 100 years. Without a toilet on site, the residents of Qumran would have had to relieve themselves beyond the walls of their settlement. While this does not necessarily solve all of the problems raised thus far, it does suggest that a diachronic approach may be of some use in this discussion.

The primary difficulty in reading the relevant archaeological evidence through a diachronic lens, however, is that Zias' discovery of human faeces to the northwest of Khirbet Qumran has yet to be dated. The complete absence of any artefacts from the so-called Qumran latrine places us in the unfortunate position of being unable to assign a relative chronological date to Zias' discovery or, for that matter, to connect this discovery to the site of Khirbet Qumran. Although we concede that the parasitological evidence recovered by Zias would seem to confirm that the area in question was used as a latrine, the evidence that is currently at our disposal makes it impossible to determine whether the latrine is two years, two hundred years, or two thousand years old.

To Zias' credit, he does acknowledge that the dating of the parasitological evidence is problematic:

that she no longer understands 4Q472a as referring to the covering of excrement. Magness agrees that the new reading of this fragmentary text is significantly different than its original publication in DJD 35 and she no longer cites this text when discussing the issue of excrement at Qumran.

(30) Humbert and Chambon, *Fouilles de Khirbet Qumran et de Ain Feshkha*, 309.

(31) For a description of the earthquake of 31 BCE, see Josephus, *Antiquities* 15.121.

(32) Humbert and Chambon, *Fouilles de Khirbet Qumran et de Ain Feshkha*, 72-73.

Since we cannot rely on other archaeological evidence (e.g., ceramic or architectural) from Area A, the question of dating these particular parasites is certainly valid, especially since the Bedouin are known to have periodically inhabited the region. However, according to Araf Abu-Rabia, Professor of Anthropology, Ben Gurion University, himself a Bedouin, the Bedouin do not bury human waste after going to the toilet. They simply leave the waste on the surface and subject to rapid desiccation in the desert. (33)

Unfortunately, the argument that Zias employs in order to circumvent the issue of dating is tenuous. With all due respect to Professor Abu-Rabia, it is impossible to speak for all Bedouin at all times on this matter. In any case, Professor Abu-Rabia's statement fails to address the issue of dating the parasitological evidence recovered from the Qumran plateau. Even if the statement above concerning the Bedouin's toilet practices were completely accurate, we would still have to account for any number of individuals who have lived and operated in this region over the last two millennia. In particular, the Roman garrison that took up residence at Qumran after its destruction in 68 CE and the various archaeological teams that have worked in this area since the late 1940's would need to be accounted for and removed from the pool of possible candidates. In short, the issue of dating the parasitological evidence recovered by Zias will remain an unresolved problem until a thorough excavation of the area in question is conducted and an official field report is published. (34)

Maximalism vs. Minimalism

Having discussed the relevant textual and archaeological evidence we are now in a position to offer two interpretations of Zias' discovery. From the maximalist point of view, the discovery of human faeces to the northwest of Khirbet Qumran has verified the witness of Josephus and the Dead Sea Scrolls on the matter of defecation. Not only has Zias recovered human parasitological evidence to the northwest of the site of Khirbet Qumran, which would appear to be in at least partial agreement with the *Temple Scroll*, *War Scroll*, and *Deut* 23:13-15, but the evidence in question was also located some 20 to 30 centimetres below the surface of the topsoil, (35) a fact that would seem to correspond with Josephus' description of the Essenes and

(33) Zias, Tabor, and Harter-Laiheugue, "Toilets at Qumran," 636-37.

(34) A similar criticism is made by Magness regarding the soil samples and parasitological evidence from L51: "We do not know how much (if any) of the original soil has remained in the room (contaminated or not). And even if the soil samples that were analyzed were ancient, there is no way to determine whether they come from a sectarian context or from a post-68 Roman occupation." Magness, "Toilet Practices at Qumran: A Response," 278.

(35) Zias, Tabor, and Harter-Laiheugue, "Toilets at Qumran," 634.

their unusual habit of burying faeces in remote locations. (36) From the maximalist perspective, even the presence of a toilet within the confines of Khirbet Qumran does not present us with a significant problem. After all, one toilet would clearly be inadequate for a large number of people who were living and working in a remote location like Qumran. Certainly, accidents happen, and one could quite easily see how the Qumran community could have made accommodations for those individuals who were unable to relieve themselves in a designated place beyond the walls of the site.

A minimalist reading of the textual and archaeological evidence is, as one might expect, far less optimistic than the maximalist interpretation. In particular, the minimalist cannot look past the fact that there is, at the present time, no way to date the parasitological evidence from the Qumran plateau. Without a relative chronological date the human faeces recovered by Zias cannot, and should not, be connected to the Qumran community or to the site of Khirbet Qumran. The minimalist will also take issue with the fact that the textual evidence, which is anything but consistent on the issue of defecation, has been used in a highly selective manner by Zias. Not only have the issues of genre and dissonance been discounted or ignored, but also the textual evidence from the Dead Sea Scrolls and Josephus, as well as the Qumran/Essene hypothesis, has been employed by Zias as a sort of patchwork map for locating and interpreting the archaeological evidence from Qumran. There are at least two problems with this approach: (1) it is based on the tendentious assumption that the rules in the *Temple Scroll*, the *War Scroll* and Josephus were applied directly to the site of Qumran; and (2) it forces us to read the material culture from an ancient site in light of the textual witness rather than allowing the archaeological evidence to speak for itself. These difficulties are most apparent with regard to the discovery of a toilet within the walls of Qumran. In particular, this presence of a toilet within the confines of Qumran presents us with compelling archaeological evidence that the inhabitants of this site, be they Essene or not, were not as dogmatic or as stringent about their toilet practices as the textual witnesses that have been employed by Zias would lead us to believe.

Middle Ground?

Putting aside, for the moment, the absence of a relative chronological date for the so-called Qumran latrine, it may be possible to offer a diachronic reading of the archaeological and textual evidence at our disposal. Before we do so, however, we must ask the reader to bear in mind the highly speculative nature of this proposal.

(36) See n.10 above.

To begin with, it must be said that the Qumran community's evolution from a group that separated itself from the Temple establishment into a group that seems to have understood itself as being a human replacement for the Temple did not happen overnight. This monumental shift in the locus of purity, which is not unlike rabbinic Judaism's shift from a religion of the Temple to a religion of the Book, would have taken a significant amount of time to accomplish and it stands to reason that the community's position on defecation would have evolved as well. For the first 70 to 100 years of the community's existence, a toilet was present within the walls of Qumran (i.e., from c. 130-100 BCE to 31 BCE). This observation suggests that the community did not adhere to the latrine regulations of the *Temple Scroll*, the *War Scroll*, and *Deut* 23. However, by at least as early as the earthquake in 31 BCE, (37) and perhaps some time before this catastrophic event took place, the community's self-understanding may have shifted from one that did not see the rules of the *Temple Scroll*, the *War Scroll*, and *Deut* 23 as being relevant to one that understood them as being binding. (38) As the group's identity evolved into a progressively stringent "temple of men," it is possible that the act of defecating outside of the "city of the temple" (i.e., Khirbet Qumran) became mandatory. As this practice became more and more standardized within the Qumran community it is conceivable that, by the time Josephus had come in contact with the Essenes, it had evolved into a practice that had been embraced by the larger movement of which the Qumran community was a part.

While appealing from a diachronic perspective, the reader is reminded of the highly speculative nature of the aforementioned proposal and the fact that it is prone to many of the same criticisms outlined above. In particular, not only does this proposal ignore the issues of genre and dissonance between the Scrolls and Josephus, but it also disregards the absence of a relative chronological date for the so-called Qumran latrine. Until the parasitological evidence is dated and a connection between Khirbet Qumran and the human excrement to the northwest of the site is established, the diachronic theory of-

(37) Although highly tentative, one wonders if there is a direct connection between the earthquake of 31 BCE and the absence of a toilet within the walls of Qumran after this date. Specifically, given that the fault line from this geological event literally bisects the *miqveh* of L.48-49 and the toilet in L.51, it is theoretically possible that the inhabitants of the site could have interpreted the destruction of these installations as a form of divine disapproval. A theological interpretation such as this could have prompted the inhabitants of the site to embrace a more stringent position such as those that are evidenced in the *Temple Scroll*, *War Scroll*, and *Deut* 23. For a discussion on the relationship between earthquakes and theophanies, see Richard Baukham, "The Eschatological Earthquake in the Gospel of John," *Novum Testamentum* 19/3 (1977): 224-233.

(38) Or, at the very least, into a community that understood the rules of *Deut* 23 as being applicable to their wilderness "camp" at Qumran.

ferred above will remain just that: a theory, though a theory that is, at this juncture, just as plausible and convincing as the one forwarded by Zias.

Conclusion

Contrary Zias' claim that the parasitological evidence from the Qumran plateau "bolsters the Essene hypothesis by corroborating the descriptions of this distinctive toilet regimen in both the scrolls and Josephus," (39) we find no evidence to support such a conclusion. Although we agree that the area in question seems to have been used for the disposal of human waste, it is premature to conclude that this evidence corroborates the witness of the scrolls or, for that matter, Josephus' descriptions of the Essenes. As Davies has noted: "When observation and theory become mixed up in the process of describing a site, the unformed reader will likely be mislead." (40) In the case of the Zias' study, theory has become virtually indistinguishable from observation and only a complete excavation of the area in question will enable us to discern whether or not we have been mislead or enlightened.

Ian WERRETT

(39) Zias, Tabor, and Harter-Laiheugue, "Toilets at Qumran," 631.

(40) Davies, "How Not to Do Archaeology," 206.

ON THE LANGUAGE OF '*THE VISION OF GABRIEL*'

Background

ADA YARDENI and Binyamin Elitzur recently published a fine *editio princeps* of a “prophetic text on stone from the first century.” (1) In their edition, they write that, “Since the text is placed in the mouth of Gabriel, it is perhaps possible to name the text ‘Vision of Gabriel’.” On the basis of what is written in the text, and what the editors say, I have adopted this name. (2) The stone consists of “two columns, in the style in which Torah scroll is written”; (3) the horizontal lines and the vertical lines which end each column “are incised sharply, in a manner very similar to the incisions in the Dead Sea Scrolls.” Within is found “a literary composition unknown from other Jewish sources.” The text is spread out over 87 lines. (4) The editors assign the text to the genre of prophetic literature. (5)

* I would like to thank my friends Binyamin Elitzur, Devorah Dimant, David Talshir, Haim A. Cohen, and Michael Ryzhik, and my brother Meir Michael Bar-Asher, all of whom read this article and made some comments.

(1) See Yardeni and Elitzur. They have also submitted an English version of the article for publication (Yardeni and Elitzur [English version]). In this second version there are only very few changes from what was presented in the original Hebrew version; when the occasion arises, these will be pointed out below. There is also a brief notice in Yardeni 2008.

(2) Yardeni and Elitzur, 162. Knohl, too, has adopted the name.

(3) This quotation and the two that follow in this paragraph are from Yardeni and Elitzur, 156.

(4) Lines 1-10, 32-56, and 59-64 are almost entirely obliterated, and very little can be read within them. Lines 66-87 are damaged at their ends, and there are other smaller lacunae in other lines, as well.

(5) As an appendix to this article I have attached the text as read by Yardeni and Elitzur. I am deeply indebted to them for agreeing to have it presented here as an appendix to my article.

§2 Yardeni and Elitzur point out one citation from the book of *Haggai*, (6) and also call attention to formulations within the text drawn from the books of Zechariah, Daniel, and other biblical books, as well as formulations known from later Hebrew literature (“such as the Hekhalot literature, piyyutim, Talmudic literature, and midrashim” (7)). There are also expressions within the text which have no parallels elsewhere. The editors determine based on the language of the text that it dates from approximately the first century BCE, and, based on its script, that it was copied in the same era. (8)

Following the general presentation of the text, Yardeni and Elitzur’s study contains three important parts: (a) an edition of the text (p. 158; between pp. 159 and 160 they also present a “transcription and decipherment”); (9) (b) a description of the text, including short discussions of the contents, its relationship to other compositions, and parallels among them; scattered among these are some short comments on the language; (c) a discussion of the paleography, including a detailed description of the script. (10)

§3 After a first draft of this article was written, I became aware of a thorough article about the text written by Israel Knohl; he had traveled to Zurich in order to read the text itself in person. Knohl’s article has since been published in *Tarbiz*. (11) Regarding the basics Knohl accepts the reading of Yardeni and Elitzur, and even singles out certain details for praise, although regarding a number of points he proposes readings other than theirs. (12) Knohl’s study is devoted on the whole to the content of the text (and at some points he proposes interpretations which are speculative only), but also contains a number of comments on the language of the text. (13) It should be

(6) This is a nearly verbatim citation of *Hag* 2:6 (see below, §27).

(7) Yardeni and Elitzur, 156.

(8) *Ibid.*

(9) The editors also include photographs of the text on pp. 159 and 162.

(10) This discussion (pp. 162-166) is the work of Dr. Ada Yardeni, and includes a detailed comparison with the script in two Qumran texts. This convincingly demonstrates the similarities with the script of the Qumran texts, but the editors skimp on linguistic discussion throughout, and suffice with scattered brief comments, as mentioned. For all intents and purposes, there is no linguistic discussion in the article.

(11) When I became aware of the new readings proposed by Prof. Knohl, I asked his permission to study his article, and he was kind enough to provide me with a copy prior to its publication. I also gave him an early draft of the present article (this was all in January 2008). Later he and I discussed our two articles in detail when we met at the University of Vienna (in February 2008, at a conference on the Dead Sea Scrolls at which Prof. Knohl lectured about the text), and I also provided him with a revised and expanded version of my article. I thank Prof. Knohl for permission to read his article prior to publication, and for the discussion with him.

(12) He writes: “After inspecting the text, I learned to appreciate the good work done by the editors in transcribing and reading the text. Even so, at a few points I would like to suggest corrections to their readings” (Knohl, 304 n. 4).

(13) Here are some of Knohl’s linguistic notes: (1) According to his reading, the rare word טָהָב (damp) is found in l. 40 (p. 326 n. 95). If this reading is affirmed,

emphasized that among his new readings are the adoption as certain of a number of letters and words which the first editors had labeled doubtful. (14) Knohl includes a new edition of the text in his article. (15)

§4 Yardeni and Elitzur claim that "the language is closer to Mishnaic Hebrew (MH) than to Biblical Hebrew (for example, there is no use of the *waw* consecutive in either past or future usages), and Aramaic influence is evident." (16) On the other hand, they also note that "the text includes many biblical expressions." (17) In short, both

this is the earliest attestation of the word in Hebrew. (2) He also claims to have found the reading לובנסד (*lwbnšyd*). in l. 22 (p. 308), explained as (לובן סיד lit.. whiteness of plaster). (In the first possibility, סיד is the noun שִׁיד/סִיד, with which לובן is in the construct. For the second, although Knohl does not say as much, his intent is apparently to suggest that the form is a defective spelling of one of the two attested forms of the Qal passive participle of the medial *waw* root סוּד סִיד and סוּד. סִיד would be comparable to שִׁים in the verse אֵיתָן מוֹשְׁבָהּ וְשִׁים בְּסֻלַּע קִנָּה (*Num* 24:21), in which שִׁים means "placed." The other form, סוּד, would be comparable to forms such as סוּג (*Prov* 14:14), סוּגָה (*Song* 7:3), לוּטָה (*1 Sam* 21:10), שוּמָה (*Qere*, *2 Sam* 13:32; Ketiv: שִׁמָּה), and Mishnaic Hebrew forms like צוּרָה ("drawn") in the line שַׁעַר הַמִּזְרָחִי וְעָלִי צוּרָה שוֹשֵׁן הַבִּירָה צוּרָה (*m. Mid* 1:3). (3) Knohl's explanation of the verb in the phrase אוֹ תַעֲמִדוּ (l. 85) as "then you shall rise to life," based on parallels in *Ezek* 37:10 and *Daniel* 12:3, is particularly attractive. (4) The suggestion Knohl cites in the name of Shelomo Naeh (p. 307, with n. 20) to understand קדשה in the phrase יִשְׂרָאֵל קדשה (l. 19) as קִדְשִׁי is interesting. He apparently means to take the הֵּ as a marker of the vowel [e], which is the result of a shift of [i] > [e], for which comparable forms can be found in the Naḥal Se'elim documents (עֵין גִּדִּי=עֵינִידָה], בעֵלָה [=עֵלִי= my husband], and more [cf. Cotton and Qimron 1998, 110-111]). Still, it should be noted that the rest of the forms in the text which have 1cs suffixed pronouns (possessive or objectival) are written with a final *yod*: שְׂאֵלִי (11 = שְׂאֵלִי for שְׂאֵלִי?, [you]ask me), עַבְדִּי (16, my slave), שְׂאֵלִי (21 = שְׂאֵלִי ask me!), עַמִּי (my people).

(14) The editors note explicitly that many of the readings proposed are doubtful (see below, n. 19, and the note which Yardeni and Elitzur preface to their edition of the text, included below before their edition in the appendix).

(15) Knohl edits only lines 11-42 and 54-87 (indeed in ll. 1-10 and 43-53 only scattered letters and a few complete words can be read). Below, following Yardeni and Elitzur's edition, I have included Knohl's edition, as well. I thank him for permission to include his edition as an appendix.

(16) Yardeni and Elitzur, 156. In fact, throughout the article are sprinkled the editors' remarks on the language and its affinities to MH: for example, with regard to מֵה זֶה (l. 31) with the MH form of the demonstrative pronoun in place of BH זֶה (p. 160); the use of the relative pronoun שֶׁ in place of biblical אֲשֶׁר (p. 161), as in שְׂהוּא (l. 36), שְׂרָאִיתִי (l. 71), שְׁלִקְחָתִי (l. 83; in the reading of Yardeni and Elitzur the שֶׁ and the final two letters are doubtful; in Knohl's reading only the final two letters are doubtful). They also comment on linguistic features of the text which are the result of Aramaic influence. For example, they point to מִן לִפְנֵי (l. 16) and מִן לִפְנֵיךְ (l. 17), calqued from Aramaic מִן קִדָּם – although the forms מִלִּפְנֵי (21) and מִלִּפְנֵיךְ (79) with prefixed (מִן) are also found (cf. p. 160). It is worth noting that מִן (without the assimilation of the –ן to the following word) is found prior to nouns in Chronicles, as well, in addition to the Bar Kokhba letters and reliable manuscripts of rabbinic literature (cf. Kutscher 1977, 55). On the other hand, the possibility raised by Yardeni and Elitzur (p. 159: they are careful to preface it with "maybe") that the form מִלֵּאכָה (28, 31) is Aramaic does not seem plausible to me; see below, §§16-18.

(17) Yardeni and Elitzur, 156.

publications contain important comments on the linguistic aspects of the text, but neither has exhausted the issues involved. (18)

Clearly a full study of the language of the text, including all preserved lines, would be very valuable, in order to reach firmer conclusions about the language and the date of the text. In this study I will deal briefly with the orthographic practices found in the text and compare them with contemporaneous and other texts. I will then discuss certain issues of phonology and morphology, and study in detail two distinctive linguistic features attested here. I believe that even though these details do not add up to provide a thorough linguistic study of the text, they do address the primary features which distinguish its language, and they provide support for the view of Yardeni and Elitzur, as well as Knohl, that the text dates from late Second Temple times. (As mentioned, they go farther and specify the first century BCE.)

On some orthographic practices

Defective spellings

§5 It is noteworthy that the writer often utilized defective spellings even when writing words with long vowels. (19)

- a. The text spells words with [i] vowels without a *yod*, even when the vowel is a long [ī]. For example, we find **ואגדה** (12) (20) = **ואגידה** and **הציץ** (31) = **הצִיץ** (Yardeni and Elitzur read thus, carefully introducing the suggestion with “maybe,” (21) but Knohl does not hesitate to accept this reading (22)). The name **דוד** is also written defectively, **דוד** (16, 72), and not **דויד** in accordance with the common practice of the late biblical books and the Qumran texts. (23) According to Knohl’s sug-

(18) The content, however, has been discussed at length by Knohl (and it should be noted that two other scholars have informed me that they are planning on publishing studies of the text’s contents).

(19) As mentioned above (n. 14), Yardeni and Elitzur are careful to note (p. 158) that the readings of many letters are in doubt. These appear in outline in their edition and here. Obviously this study will draw conclusions only on the basis of readings firmly attested. If a point does not depend on the doubtful letter, its status as doubtful will not be indicated here. At this point it is worth mentioning that Dr. Ada Yardeni deserves great praise for her decipherment of the text; anyone who studies the photos and her article will quickly see how difficult the decipherment was.

(20) The numbers in parentheses indicate the lines in the text in which the forms mentioned are found.

(21) Yardeni and Elitzur, 160.

(22) Knohl, 311.

(23) In late biblical books only the spelling **דויד** is found (more than 250 times), and the old spelling **דוד** is nearly never attested (the single exception is in the superscription of *Qohelet*, which opens, **דברי קהלת בן דוד**). In the *Great Isaiah Scroll* from Qumran, too, the spelling **דויד** predominates (see Kutscher 1959, 5).

gested reading, which sees לובנסד in l. 22 as לובן סיד, the writing סד (= סיד) also belongs here. (24)

- b. Defective spelling is also found in the text for the long vowel [ē] ← [ay], after the *nun* in the word לפנך (17) = לִפְנֶיךָ. (25)
- c. The vowel [ō], represented in Tiberian orthography with a *holem*, is also spelled defectively in the text: הגאים (13) = הגואים; (26) אלהי (e.g., 1, 18, 20, 26) = אֱלֹהֵי; אלה (26) = אֱלֹהֶ; צבאת (68, 69, 84) = צִבְאוֹת (this is the spelling in the final three attestations of this word in the text, but in the first six attestations the feminine plural ending -ōt is written plene, צבאות [11, 18, 20, 26, 29, 58], (27) and the word המרכבות [26] is written plene, as well). Defective spelling for [ō] is found in two additional words: for the vowel following the *dalet* in בגדלות (12, 33) = בַּגְדֻלוֹת, (28) and in the word שמר (33) = שׁוֹמֵר (although the reading is doubtful, (29) and also attested are participles with a *waw*: עומד [22], עושה [74]).
- d. According to Knohl's readings, the long vowel [ū] is also written defectively in the text. For example, the form ינחו (27) = יְנוּחוּ (Yardeni and Elitzur read only the first two letters with certainty: ...ינ); (30) דם טבחי ירשלם (57) = דֵּם טְבוּחֵי

(24) See the detailed discussion above, n 13...

(25) In the 2nd person plural pronominal suffixes, the low [æ] vowel is the reflex of an original [e] vowel, due to partial assimilation with the low [ā] in the final consonant.

(26) The writing גואים is found in the *Great Isaiah Scroll* around 50 times (Kutscher 1959, 404), and הגואין is found in one of the Bar Kokhba letters, too, the spelling הגואין is found (Kutscher 1962, 58. Yardeni and Elitzur (p. 162) cite the example from the Bar Kokhba letter, based only on Yardeni's publication of the relevant text).

(27) I have not included here the attestations of this lexeme whose reading is not certain (e.g., the example in l. 40) or which is restored by the editors (צבאות in l. 57). In any event, it is worth emphasizing that the copyist of the text wrote the word plene at first (six times), and then changed his practice and wrote it defectively (three times). Might this be evidence that the copyist was working from two different copies, written by two different scribes?

(28) Yardeni and Elitzur (159) cite the phrase באגדה בגדלות (12) and draw attention to its parallel in Jeremiah (33:3), ואנידה לך גדלות. On p. 160, on the other hand, they vocalize the word גדולות (and accidentally refer to *Jer* 30:3 instead of 33:3, mentioned above). If they really meant to take the form גדלות, they could have referred to *1 Chron* 17:21: לשום לך שם גדלות. It is clear, though, that this was a mistake, and the initial suggestion (on p. 159), which relied on the close parallel with the verse from *Jeremiah*, is correct. Knohl, however, does read גדלות (he refers to the verse in *1 Chron*). The two readings – גדולות and גדלות – both yield the same meaning, since the plural form גדולות may also be an abstract noun, but this cannot be elaborated here.

(29) It is marked as doubtful in both editions of Yardeni and Elitzur and Knohl.

(30) In the English version of their publication, Yardeni and Elitzur read ...וני (with an initial *waw* instead of a *yod*). Both Yardeni & Elitzur and Knohl mark the letters read in this word as doubtful.

e. The name of Jerusalem, which at this time was already pronounced [y^erušalayim], (34) is also written defectively, without the *yod*: ירושלם (e.g., **12, 27, 39**). (35) It should be mentioned that reliable manuscripts of Tannaitic literature also adhere to the defective spelling of this name. (36)

a. Additionally, some originally *short* vowels are written plene, such as קִטּוּט (24), which is almost certainly the spelling of קטוט; (37) כּוּל (13), ולכוּל (28) = כָּל, וּלְכָל.

b. Also worthy of being singled out is the spelling כֶּה—for the 2ms form of the suffixed objective pronoun, in the word לִכְה (21) = הָלְי (to you). This spelling is found in the Bible and is very common in the texts from Qumran. (38) And if we ac-

(38) See Kutscher 1959, 349-350. See also the important study, with a large collection of data from scores of sectarian texts from Qumran, by Emanuel Tov (2007, 337, 341; see esp. col. 18 in the detailed tables on pp. 342-352).

cept Knohl's reasonable reading of *המלאך הוא בסמך אל תירה* (22-23), (39) and his interpretation of *תירה* as another spelling of *אל תִּירָא* (don't be afraid) there is evidence for the preference of ה rather than א for marking final [ā] vowels.

The spellings of final ם- and ן-

§7 a. There are more than 10 attestations of the masculine plural ending spelled ם-. The least informative attestation is in the word *לאלפים*, in the phrase *לאלפים חסד עושה* (74). Since this phrase is taken from the Ten Commandments (*Exod* 20:5; *Deut* 5:9), its orthography can be expected to remain stable whenever it is quoted. But the spelling ם- also appears on many other well known words: *הגאים* (13) = *הגואים* (68), *רבים* (68), *נביאים* (70), *קדושים* (76), *צרים* (81), and especially the name *אלהים* (11, 25, 29, 38); (40) the pseudo-dual form *שמים* is also spelled this way (25, (41) 73). Other words, without the masculine plural ending, also preserve the orthography with final ם: *העולם* (65), *שלום* (66), *המקום* (72).

b. Evident, on the other hand, is the growing tendency to utilize ן for the plural and dual endings. More than 10 examples of the plural ending are written with *nun* ן: *והשבין* (15) = *והשבין*, *גביאין*, *ארבעין* (and the elders), (42) *והחסידין* (16), *לשלשת ימין* (19, 80) = *ימים* (28), *האחרין*, *השקרים* (81) = *השרין* (75), *רועין* (68), *אוהבין* (66), *בטוחין* (15) = *שתין* (15) = *שתיים* and *שנין* (30) = *שניים*. The same is true for the 3mp pronominal suffix, which is also written with a *nun*: *שלהן* (67).

Summary of the orthographic discussion

§8 Let us summarize the issues of spelling which have been discussed here.

- a. It must be said that the tendency towards defective spellings of the vowels described above in §5 is most characteristic of biblical Hebrew, although defective spellings of long vowels are found in the Bar Kokhba letters, as well, as Kutscher

(39) Yardeni and Elitzur read *המלאך הוא כסמך אל תורה* (with a *kaf* as the initial letter in *כסמך* and a *waw* as the second letter of *תורה*).

(40) I have not cited the form *גנים* (= *גנים*? gardens) in l. 18; Knohl (307) detaches the final ם from this word and reads it together with the following word, so instead of *גנים וכרים* as read by Yardeni and Elitzur, he reads *גני מבוכרים* (= *גני מבוכרים*?).

(41) In the occurrence in l. 25, the ך and the ם are doubtful.

(42) This biblical word is written as it is found in BH: *שָׁב* (*Job* 15:10). It is written the same way (*שבים*) in *Ben Sira* (8:9 and twice more; see the concordance in the edition of the Academy of the Hebrew Language). This is also the orthography in biblical Aramaic: *לְשָׁבָא* (*Ezra* 5:9 and four more times). In Qumran the word is found only in Aramaic, written twice with a *sin* (4Q562 2 4, 2Q24 4 13) and once with a *samekh* (4Q201 3 3).

showed years ago. He established that there are “a multitude of examples of defective spellings of long vowels,” (43) such as *עלתי* (= *עֲלִיתִי*), *אלנו* (= *אֲלִינוּ*), *אכלין* (= *אֹכְלִין*), *דאגין* (= *דֹּאגִין*), and *ידע* (= *יֹדַע*). In this respect, then, our text differs both from the Qumran texts (which tend towards extremely full orthographies), (44) and from the practices attested in the manuscripts of rabbinic literature.

- b. The plene spelling of the 2ms pronominal suffix as *כה*—, attested once in our text in the word *לכה*, is known from the Bible, but is very common in the Qumran texts. The spelling *כול* is also characteristic of the texts from Qumran.
- c. On the other hand, short [i] followed by gemination is not written with a *yod* at Qumran. (45) This spelling is found in the word *קיטוט* (= *קִטּוּט*), however, and this practice is typical of the manuscripts of rabbinic literature (Tannaitic and Amoraic). (46)
- d. The multiplication of plural forms with final-*nun*, as well as the use of the *nun* in dual and 3rd person masc. plural forms, also brings the orthography of the text closer in line with the practices of rabbinic manuscripts.
- e. The use of *he* ה as a *mater lectionis* for final [ā] (*תירה* in place of *תִּירָא*) also matches what is known in the most reliable rabbinic manuscripts, as well as Palestinian Aramaic. (47)
- f. The spelling of the word *והשבין* (= *וְהִשְׁבִּין*) “and the elders”) with a *sh* accords with biblical spelling.

In light of (c), (d), and (e), it is clear that the editors were correct in aligning the language of our text with that of the Mishnah (and with Aramaic). Still, the significance of features (a), (b), and (f), which divide it from Mishnaic Hebrew, should not be minimized.

And there are yet more orthographic issues regarding the text which we have not presented here.

§9 To sum up: the orthography of every ancient text is unique, due to the personal habits of scribes, the varieties of practices taught in scribal schools, and features which accrue through copying. This is certainly true for the *Vision of Gabriel*. When we take into consideration the conservative practices which align this text with the text of the Bible, the few features which are comparable to the Qumran texts, and the features which are well-known from Mishnaic Hebrew, it

(43) Kutscher 1962, 66; what Kutscher wrote about the spelling in Bar Kokhba's letters is of fundamental importance and should be seen in the original.

(44) In Kutscher's expression, *מלא דמלא* “fullest of the full” (ibid., 66).

(45) Cf. Qimron 1976, 53-55.

(46) Cf. Bar-Asher 2006b, §16-17 (pp. 21-23); Breuer 2002, 33-37.

(47) Cf. Kutscher 1977, 178; Breuer 2002, 27-37.

seems that we can agree with the view that the text was written and copied in late Second Temple times. (48) In my opinion, this view can be further supported by the issues of phonology and morphology which I will now discuss (§§10-19). (49)

Phonological and morphological features

§10 With regard to phonology and morphology, as well, there are a number of interesting features in our text, three of which we will now discuss. The first, the loss or merger of the laryngeal and pharyngeal consonants, is shown by a number of pieces of evidence, some more and some less certain. We will discuss each piece of evidence individually, and only at the end will we attempt to make sense of the data when taken as a whole. The second feature relates to both phonology and morphology, but describing it is somewhat problematic. The third feature is a detail of morphology, and its nature and significance are clear. I will now discuss the details of these issues.

The weakening of the gutturals

§11 Investigation of the text shows that a number of words indicate that the gutturals were pronounced indistinctly or not at all: this is reflected in the cases in which gutturals are interchanged or omitted entirely.

Omission

- a. There is one example of the omission of an א: in l. 77, the text reads *אני גבריאֵל המל*, and there is space for only one letter in the lacuna following *המל*. It is most reasonable to assume that the letter was the final כ, and that the א was never written in the text; instead, the word was written *המל<ך*. On the other hand, in ll. 22-23 the word appears written with the א, (50) in the phrase *המלאַךְ הוא בסמכך*. (51)
- b. Twice the word *לִידוּד ה: (אֱלֹהִי) אֱלֹהֵי* is written without the ה: *ידוד* (84). *צבאת אל>ה<* (53) *צבאת אל>ה< ישראל* (52). The first editors “restored” the missing ה in both of these occurrences, but it seems to me that the other spellings of this word in the text *אלהי* (13, 18, 20, etc.) are historical spellings.

(48) As mentioned, the editors saw fit to be more specific, and assign the text a date of the first century BCE.

(49) See further below, §§37-38.

(50) There are two other instances of the spelling of the word with the א: *מלאכה* (28. 31). This form is the focus of our discussion below, §§16-18.

(51) I am reading here *בסמכך*, with Knohl (cf. above, §6b).

(52) As is the practice, I have replaced the two *hēs* in the divine name with *dalets*.

(53) See the above n.

The two spellings without the ה are probably not spelling mistakes; it seems unlikely that the same error would appear twice in the same text. It would then appear likely that the two defective spellings represent how the word was actually pronounced in the Hebrew of the scribe: [eloye], or something of the sort, with a [y] glide between the vowels [ō] and [ē]. The *yod* which was originally a *mater lectionis* for the [ē] vowel, then, now indicates a consonant. (54) (There is no orthographic reflex of the [ō] vowel in any of the spellings of this word in the text.)

Interchange

§12 Interchange of guttural consonants is seen in two words. The first is in l. 64, where the word **הביב** is apparently found, but both the original editors and Knohl are unsure whether it is written **הביב** or **חביב**: Repeated study of the photograph and the hand copy included by Yardeni and Elitzur with their edition supports the reading specifically with a ה: **הביב**. (55) From this one can conclude that the pronunciation of the ה had weakened, and the word was pronounced [abib] or something of the sort. (56)

§13 The second example is more problematic, but if we accept Knohl's reading and interpretation, an important instance is found in l. 80. There Yardeni and Elitzur read ...**לשלושת ימין ח**, and left the final three letters of the third word unidentified. (In the English version of their publication, however, they do identify the second letter, and read ...**חא**.) Knohl, however, identifies all four letters, and reads **חאיה** **לשלושת ימין חאיה**; he identifies the word as the imperative of the root **חיה**, i.e., **חיה** "live!" (57) Knohl himself does not rush to accept his suggestion, but in the end he does; (58) in nn. 54-55 he refers to the

(54) It might be objected that we would then expect a spelling with an **א**, **אלאי**, akin to the spelling **הנאים** for **הגיים** (= **הגיים**); such a spelling would have unambiguously revealed the loss of the [h] in the word **אלהי**. But such a spelling would only result if between the vowels was a glottal stop and the realization was [elo'e], not [eloye].

(55) In a personal conversation in February 2008, Dr. Ada Yardeni agreed that the reading with ה was preferable (Later Yardeni said to me that she prefers to read **חביב**, however I prefer to read **הביב**).

(56) The various traditions of Mishnaic Hebrew differ regarding the pattern of this noun: MS Kaufmann and others read it as a *qātīl* noun (**חַבִּיב**), while MS Parma B and others read it as a *qatīl* pattern noun (**חְבִּיב**). See my detailed discussion of the morphology of this word in Bar-Asher in press, §§5-6.

(57) It must be noted that in the Tiberian tradition of biblical Hebrew, the form **חַיָּה**, a reflex of **חיה**, is attested (*Gen* 20:7; *Prov* 4:4; 7:20). In the Babylonian tradition, the form is found as **חַיָּה**, however (Yeivin 1985, 711), and the feminine form is attested with a [ā] vowel (*hataf patah*), as well: **חַיָּי** (*Ezek* 16:6)..

(58) Cf. Knohl, p. 319-320 and nn. 62-64 (in n. 64 he hints at my proposal here). In August 2008 Yardeni said to me that she accepts the reading **חאיה** proposed by Knohl. (But I know that a young scholar is going to propose another different reading).

literature regarding the use of a א as a *mater lectionis* to represent an [a] vowel (59) in the Bible, in the Qumran texts, in the epigraphic material, in Mishnaic Hebrew, and in the Palestinian Aramaic dialects.

However, among all the examples cited by Knohl, not a single one is known in which an א is used to represent an [ä] vowel (*hataf patah*) in Hebrew. In every case of an א representing an [a] vowel, the vowel is a whole vowel; far more common is that the vowel is a long [ā] (*qames*), (60) such as וְקָאֵם (*Hosea* 1:14); בַּעֲאֹן (*Great Isaiah Scroll, Isa* 1:17) = בַּעֲוֹן or בַּעֲוֹן; רַבְּאֵי (Mishnah with Babylonian vocalization; *Ma'aser Shenit* 5:3-4) = רַבְּעֵי. There are also some examples of א standing for a short [a] (*patah*), such as noun כָּאֵן הַזֶּה ("golden base (pedestal)," MS Kaufmann, *Yoma* 5:3, 5:4[2x]), but there is not a single example of א standing for a reduced vowel [ä] (*hataf patah*). (61) If we add the fact that in the Tiberian tradition, the imperative form for the masculine singular is חֲיֵה, (62) it seems that the א in חֲאִיָּה does not represent an [a] vowel at all.

§14 I suggest, therefore, a different explanation of the form חֲאִיָּה. If the reading and sharp interpretation suggested by Knohl are accepted, I would suggest that this form shows that the ה was no longer pronounced, and the א was added instead. (63) If this suggestion is accepted, it would turn out that the writing חֲאִיָּה preserves both the

(59) Such as Kutscher 1959, 16-18; Qimron 1975.

(60) For details, see Qimron 1975, 336-343; I cite here only some representative examples from Hebrew texts.

(61) If the form were taken as a *Pi'el* verb, the א could be seen as standing for the [a] vowel, in the form חֲיֵה. It is true that a shift of many verbs from *Qal* to *Pi'el* is known (e.g., שָׁלַח → שִׁלַּח; cf. Ben-Hayyim 1958, 49-55), but this did not affect the root חֲיֵ. As is known, חֲיֵה (or חֵי) in the *Qal* means "to be alive," whereas the *Pi'el* form חֲיֵה means "to cause (another/others) to be alive." In the databases of the Historical Dictionary of the Hebrew Language (*Ma'agarim*), there is only one occurrence of the *Pi'el* with a non-causative meaning, in a late text which clearly does not reflect any living dialect: המַחִיָּה בְּדַבְרֵי תוֹרָה אֵין הַתּוֹרָה עִמּוֹ (*Kallah Rabbati* 80 4, 6). It seems that in this quotation, the meaning of מַחִיָּה is like חֲיֵה or חֵי in the *Qal*. That is to say that this is an instance of a *Pi'el* verb with the normal meaning of the verb in the *Qal*; the meaning of המַחִיָּה בְּדַבְרֵי תוֹרָה, then, is "one who lives in words of Torah." In parallel passages elsewhere, however, the normal phrase is הַמַּחֲיָה עִצְמוֹ; for example, לֹא הַמַּחֲיָה עִצְמוֹ עֲלֵיהָ תִּמְצָא הַתּוֹרָה אֵלָּא בְּמִי שִׁמְחִיהָ עִצְמוֹ עֲלֵיהָ (*Kallah Rabbati* 5:5), and earlier in the Bavli (*San* 111b): לֹא תִּמְצָא תּוֹרָה בְּמִי שִׁמְחִיהָ עִצְמוֹ עֲלֵיהָ. This appears later, as well: לֹא תִּמְצָא תּוֹרָה אֵצֶל מִי שִׁמְחִיהָ עִצְמוֹ עֲלֵיהָ וּמִבְּקֵשׁ חַיִּי עוֹלָם הִזֶּה (*Pirqoi* 3, *tesubot* [Tarbiz 2 (1931), 394]). תּוֹרָה בְּדַבְרֵי תוֹרָה עִצְמוֹ means "this who cause himself to live in words of the Torah," this is the normal usage of the *Pi'el*. It could simply be suggested that in the first quotation from *Kallah Rabbati*, the word עִצְמוֹ was mistakenly omitted, and the text should read בְּדַבְרֵי תּוֹרָה [עִצְמוֹ] הַמַּחֲיָה. Even if we insist, however, that the word עִצְמוֹ does not belong, and הַמַּחֲיָה עִצְמוֹ is an innovative but authentic use of the *Pi'el* with the meaning of the *Qal*, this is still not enough upon which to build a case. But all this may be for naught, since the reading חֲאִיָּה is still questionable.

(62) See above, n. 57.

(63) For the addition of one guttural letter in place of another that was no longer pronounced, see Bar-Asher 1977, chapter 8 ("The Consonants ע"אֶהֱחֵע"), pp. 367-420, and esp. pp. 397-404 ("the addition of a guttural").

historical spelling חיה as well as the phonetic spelling איה. In practice, this spelling would represent the pronunciation [eye], a development from the earlier [h^eye]. (64) But in order to be sure of this, the reading would first need to be certain.

§15 The weight of the different examples given here (המלך in place of המלאך, אלי in place of אלהי, חביב in place of חביב, and חאיה in place of חיה) varies, due to the quality of the readings in the different parts of the text. But certainly some of them show the weakening of the gutturals. However, in most of the words in the text, the letters א, ה, ח, and ע are written in their proper places. Words with etymological א in which the א is written properly include צבאות (11 etc.), האות (17); even non-etymological א is found in some words, such as הגויים (13) = הגויים. The consonant ה is properly written in words such as הארץ (25) and העולם (65), as well as others; the words הצמח (21), שלחתי (70), and others, show proper writing of the ח; and in the words עוד (24), עושה (74), and more, the ע is written properly. The deviations from historical spellings which were discussed above, however – המלך in place of המלאך, אלי in place of אלהי, חביב in place of חביב, חאיה in place of חיה – are the ones which show that the gutturals were not pronounced as they used to be. Certainty is possible only with regard to some of these, however.

Of course, it is important to emphasize in this context that the loss of the gutturals is known from the Qumran texts (65) and the Bar Kokhba letters. (66) On this background, it is significant that the data from the *Vision of Gabriel*, or at least some of the data from this text, which apparently comes from the same era, reflects similar pronunciation.

The pronunciation and the morphology of the spelling מלאכה

§16 In the following discussion, I will try to clarify the word מלאכה, which appears twice in the text.

- a. In its first occurrence, there is a difference regarding this word between the two editions of the text. Yardeni and Elitzur suggested reading, למען... מלאכה מיכאל ולכול האחרין בקשו, (27-28; the underlined letters are marked as doubtful). Regarding the interpretation of the phrase מלאכה מיכאל, the editors then debate two possibilities: the noun מלאכה may have the suffixed Aramaic definite article (מלאכה = the an-

(64) See above, n. 57. If we assume that, against the Tiberian tradition, the form was vocalized with a *hataf patah*, the spelling חאיה may be taken to represent [aye] in place of the historical חיה [h^eye].

(65) See Kutscher 1959, 42-44, and 398-403.

(66) See the examples collected by Kutscher 1977, 59 (and his conclusion there: "even within Bar Koseba's circle were men who could not pronounce the gutturals").

gel), or may have suffixed the 3ms possessive suffix (מלאכה = his angel). It should be noted that there is a difficulty in the first possibility, since the word order is the reverse of what is expected; normally in both Hebrew and Aramaic the title used in apposition to a personal name comes *after*, not *before*, the name itself: here we have מלאכה מיכאל instead of the expected מיכאל מלאכה, similar to Hebrew אהרן הכהן and מיכאל (Dan 12:1), and Aramaic קורש פרסיה (qere: פִּרְסֵאָה) (Dan 6:29). (An exception to this rule, of course, is the title המלך "the king"; attested are both המלך שלמה [1 Kgs 1:51] and שלמה המלך [1 Chron 29:24].) Still, it must be admitted that when the title in apposition to the proper noun has a suffixed possessive suffix, the word order is variable: both אבתי רבני, רבני מעירך, ירושלם, אברהם ויצחק (Gen 48:16), (Dan 9:16), and so on, as well as שלמה (1 Kgs 1:21), שלמה, בנך (ibid., v. 12), and so on, as well as שלמה, בני (ibid., v. 33), שלמה, בנך (ibid., v. 13), and more. Thus one may justify the construction מלאכה מיכאל, although the reading of Yardeni and Elitzur is not complete and not certain, and no new conclusions should be built upon it.

There is a more fundamental objection to the reading proposed by Yardeni and Elitzur, of course, and that is: why would a scribe who is writing in Hebrew, (67) and who uses the Hebrew word מלאך itself in its Hebrew form (in the line המלאך הוא בסמך [22-23]), suddenly use the Aramaic possessive form מלאכה?

Knohl, on the other hand, reads מלאכה שלוש (68) למען שלושה. In this reading it is clear that the word מלאכה is to be associated with the count noun שלושה, and that מלאכה, which is a plural noun (see below), is the head of the noun phrase מלאכה שלוש; the apposition ולכול האחרין spells out which "three messengers" are meant.

- b. The form מלאכה is also attested in a parallel expression in ll. 30-31: ... מלאכה אל [שב]עה חמשה ששה (here the first editors and Knohl agree on the reading).

(67) It is true that the first editors have shown Aramaic influence in the text, such as the Aramaic calque מן לפניך (17), which reflects מן קדמך (see above, n. 16). But there is a qualitative difference between a loan-translation from Aramaic, in a context in which Hebrew and Aramaic are in constant close contact, and the adoption of a purely Aramaic grammatical form within a text otherwise written in fine Hebrew.

(68) In the first version of Knohl's article, he wrote that the text reads שלושה, but in his edition he writes שלוש. In an e-mail message in mid-February 2008, Prof. Knohl informed me that he now reads שלוש. In the hand copy published in Yardeni and Elitzur's article, however, one can see that the reading should clearly be שלושה, and in fact Dr. Yardeni confirmed the reading שלושה for me in our conversation of February 2008. In the Hebrew edition of Yardeni and Elitzur's publication, however, they did not identify any of the letters in the word שלושה, and even in the English edition they identified only the letter ל (finally Knohl adopted my proposal to read שלושה).

§17 How are we to explain the form מלאכה after having rejected the suggestions offered by Yardeni and Elitzur? Knohl, who read שלושה מלאכה in the first occurrence, took the *he* ה as representing the diphthong [ay], (69) similar to the form עלה in *Papyrus Se'elim* 13, ll. 9-10: וקים עלה אנה שלמציין. (70) There the writing עלה is correctly interpreted as representing a reflex of the 1cs form עלי, and according to Knohl the writing מלאכה is to be interpreted as שלושה מלאכי. In one respect, Knohl's view is not sufficiently precise: the ה does not represent the diphthong, but the vowel [e] after the diphthong has contracted, and so the proper reading would be מלאכה, similar to עלה ← עלי. (71) Although such orthography (ה for [e]) is attested in Aramaic, the text's editors found good parallels within Qumran Hebrew, as well, such as the 2fs forms חלקכה, גורלכה, and so on (*Isa* 57:7), with the [e] in place of [i] vowels, standing for MT forms חלקכי*, גורלכי*, שפכתי*, and so on. (72)

Still, it should be noted well that we have no attestations of the contraction of the [ay] diphthong in the 1cs suffix within Hebrew, we have it only within Aramaic. Therefore, although Knohl's suggestion should not be ruled out, I would recommend a different suggestion which seems more reasonable to me

§18 I would suggest that the orthography מלאכה represents an alternative: the form מלאכי/מלאכין (מלאכי) to מלאכה (the standard Hebrew plural absolute form). First, it is known that in all ancient Hebrew texts, final [e] can be represented orthographically by either *he* ה or *yod* י, and interchanges between them are exceedingly common. Exemplifying this variation is the plural construct form of masculine participles of roots ending in י, such as רעה: within the Bible are attested both רעה and רעי "the shepherds of." (73) Scholars have pointed out the common practice to write י in place of ה at Qumran, (e.g., מראי for מראה) and in rabbinic literature (e.g., ארבע עשרי [MS Kaufmann, *m. Nazir* 3:6], שלוש עשרי [Talmud Yerushalmi *Sheqalim*, chap. 5, end = 49b, according to the fragments of the Yerushalmi published by Ginzberg, p. 132]), alongside the more common spellings ארבע עשרה and שלוש עשרה. (74) In short, an [e] vowel at the end of a word could be written with either a *he* or a *yod*.

(69) Knohl transcribes the diphthong as [ai].

(70) Yardeni 2000, 134; see also Cotton and Qimron 1998, 110. Knohl (n. 23) refers also to the study by Adiel Shermer, "פפירוס צאלים 13 ושאלת יכולתן של נשים, לגרש את בעליהן בהלכה היהודית הקדומה" *Zion* 63 (1998), 384-385.

(71) This emerges clearly from the linguistic discussion in Cotton and Qimron's article (1998, 110-111).

(72) See Cotton and Qimron 1998, 111.

(73) For example, compare רעה צאן עבדיך גם אבותינו גם אבותינו רעי צאן (Gen 47:3) with רעי צאן (ibid., 46:32).

(74) See Epstein 1948/2000, 1251; Yalon 1967, 33 (who cites examples from Qumran, from MH, and from the Samaritan Pentateuch). Other examples of this interchange are found within Mishnaic Hebrew in other grammatical categories, as well, such as מיכני/מיכנה, as well as in nouns which end with a *segol*, such as יבני/יבנה.

Now I can propose my interpretation of the word מְלָאכָה (= מְלָאכִי) as an absolute plural form. It would seem that the form here is similar to that in the biblical nouns חֲלוּנֵי (1 Kgs 6:4: שְׁקָפִים חֲלוּנֵי) and אֲדִירֵי כָל חִפְצֵי בָם (Ps 16:3: (אֲדִירֵי). A plural form of this sort is even found for the lexeme מְלָאךְ itself: מְלָאכֵי רַעִים (Ps 78:49)! (75) Similar forms may also be represented closer to the time of the *Vision of Gabriel* itself, in geographic names preserved in the New Testament, such as Γεθσημανέι (Matt 26:36; Mark 14:32) = גֶּת שֶׁמְנֵי, (76) equivalent to שְׁמֵנִים, (77) and this may also be the plural ending on the name Βηθφαγή (Matt 21:1; Mark 11:1; Luke 19:29) = בֵּית פְּגִי (a biform of בֵּית פִּגְיָן). (78)

I am not ignoring the fact that in none of these plural forms is the ending [e] spelled with a ה, but always with a י, such as the biblical forms חֲלוּנֵי, אֲדִירֵי, and מְלָאכֵי רַעִים cited above. However, the mutual interchanges of the *he* ה and the *yod* י in representing final [e] vowels in other grammatical categories, in addition to the use of *he* ה for original [i] vowels which shifted to [e], in the 2fs past tense verbs (קטלתה in place of קטלתי), and the 2fs possessive suffixes (גורלכה in place of גורלכי), and in the 1cs possessive suffixes (בעלה in place of בעלי), allows us to presume with no hesitation that the use of *he* ה could have spread to the plural ending, as well.

In conclusion, two points should be emphasized: (1) the geographic names שְׁמֵנֵי גֶת is attested contemporaneously with the *Vision of Gabriel* text; and (2) the form מְלָאכָה does not need to be reconstructed for the ancient Hebrew lexicon, since it is attested already in the Bible. (79)

The demonstrative pronoun אַל

§19 It is clear that the word אַל which appears in the phrase שֶׁשָּׁה ... מְלָאכָה אַל [שב]עָה (30-31) is the masculine plural demonstrative pronoun. Since for the singular we find the Mishnaic Hebrew (MH) form זוּ in this text, as well (31, 67), we would have expected to find the MH form אַלּוּ, as well. In principle one could claim that the scribe simply forgot the *waw* ו and the text should be read אַל<ו>, but there is no need, since this appears to be an example of a form known from later Hebrew, as well. This is the form found in Second Temple texts,

(75) Clearly the Tiberian vocalization (with a אָ) reflects a reading of this form as a construct form, and if it is to be taken as an absolute form, the vocalization should be with אֶ (מְלָאכֵי).

(76) See Kutscher 1977, 198.

(77) The vowel [e] is a result, of course, of the diphthong [ay] contracting. The grammars of biblical Hebrew cite (sometimes hesitatingly) a number of plural forms with this diphthong as the plural ending: חֲלוּנֵי, גִבֵּי (at a minor pause [זקן]), and more (cf. Gesenius, §86i and §87g, and see also the discussion by Zvi Rin 1961).

(78) See Kutscher 1977, 198. He is not certain, however, that פְּגִי here represents a biform of פִּגְיָן, for it may also be a reflex of [pagan].

(79) See above, n. 75.

such as *אל נולדו להרפא בנת* (*I Chron* 20:8), (80) in which *אל* replaces the more standard *אלה*, which is found in the earlier parallel passage, *אלה ילדו להרפא בנת* (*2 Sam* 21:22). (81)

Whether we accept Knohl's reading (that *מלאכי* = *מלאכה*) or my own (that *מלאכה* = *מלאכים*, a biform of *מלאכים*), *אל* cannot be an attributive demonstrative pronoun to *מלאכה* ("these messengers [angels]" or "these messengers [angels] [of mine?]"). since the word order would then have to be reversed (*אל מלאכה*). Instead, the construction must be something like *אל מלאכה [הן]* "these seven are the/my messengers." Indeed, there is no reason that the word (*המה*) (*המה*) (3mp pronoun) cannot be restored as the missing letters at the end of the line.

Two distinctive linguistic features of the "Vision"

§20 I would like to concentrate now on two features distinctive within this text, which reveal a close relationship between the text and the book of *Ezekiel*: *מושבו* (*mwšbw*) (24) and *קִיטוּט* (*qyṭwṭ*) (24). With regard to the first word, the distinctiveness has to do with the semantics of the word alone, but the second is not known from any other textual source at all, and is interesting both morphologically and semantically. I turn now to the details.

מושב/מקום (mqwm/mwšb)

§21 In ll. 23-24 we read, *ברוך כבוד ידוד* (82) *אלהים מן מושבו*, and the editors already pointed out that this is a quotation from the verse *ברוך כבוד ה' ממקומו* (*Ezek* 3:12). (83) Aside from the addition of the word *אלהים* in the version in our text, the replacement of *ממקומו* as found in *Ezekiel* with *מן מושבו* in the text is remarkable. (84)

§22 In a number of biblical passages, God (either named by the *Tetragrammaton* or referred to as *אלהים*) is spoken of as sitting on a chair/throne, or within a house/temple. For example: *ואראה את אדני יושב על כסא רם ונשא* (*Isa* 6:1); (85) *אלהים יושב על כסא קדשו* (*Ps* 47:9);

(80) The form *אל* (Qere, Ketiv: *אלה*) is attested in biblical Aramaic, as well (*Ezra* 5:15).

(81) It is also worth mentioning that the pronoun *אל* is attested five times, all with the definite article, in the Pentateuch, e.g., *הערים האל* (*Gen* 19:25), *הגוים האל* (*Deut* 7:22).

(82) See above, n. 52.

(83) See Yardeni and Elitzur, 159.

(84) For our purposes here, the difference between the texts with regard to the form of the preposition *מן* (prefixed *מ-* in MT *ממקומו* vs. independent word in *מן מושבו*) is a secondary feature. Within the text itself, both forms are attested: *מלפני* (21), as opposed to *מן לפני* (16), *מן לפניך* (79), *מלפניך* (79).

(85) For current purposes it is irrelevant whether the adjectives *רם ונשא* modify God or his throne (see, e.g., Hakham 1984-1985, n. 3 on chap. 6).

כה אמר ה' האתה תבנה לי בית לשבתי. כי לא ישבתי בבית למיום העלותי את
 הזה (2 Sam 7:5-6). (86) This type of throne
 and this type of house are, then, the proper seat (מושב) of God, and
 there is nothing surprising in what the text says, ברוך כבוד ידוד אלהים,
 מן מושבו. It is possible that one of the three verses just cited, each of
 which describes God as sitting on a throne or in a house, served as
 the inspiration for the use of מושבו by the text's author in place of the
 Ezekielian מקום. (87)

§23 It is important to stress that the replacement of ממקומו with
 מושבו shows that the words מקום and מושב were deemed to be at
 least partially synonymous. Originally the two words referred to dif-
 ferent things: the noun מקום referred to "the area in which one stands
 (קו"), (88) and then developed the primary meaning "the area in
 which one is" (like Aramaic אַתַּר, Arabic موضع, and 'place' in English
 and other languages). מושב, on the other hand, originally meant "the
 place in which one sits (ישב)," (89) and then developed the meaning
 "the place in which one is," parallel with the development of ישב to
 the meaning in verses such as וישב העם בקדש "the people stayed in
 Kadesh" (Num 20:1). (90)

There is no doubt, however, that the two words eventually over-
 lapped in meaning, as it shown by the parallel usage of the words in a
 few biblical verses. There is, for instance, certainly no difference be-
 tween the meaning of מקום in שם בכול המקומות אשר אדיחם "in all the
 places to which I banish them" (Jer 24:9) or שנים נקיון לכם נתתי אני וגם
 בכול עריכם וחסר לכם בכול מקומותיכם "I, for my part, have given you
 cleanness of teeth in all your towns and lack of food in all your settle-
 ments" (Amos 4:6), and מושב in וכלל בני ישראל היה אור במושבתם "but all the Israelites enjoyed light in their dwellings" (Exod 10:23),
 "in all your settlements you shall eat"

(86) Of course, other texts mention ה' כבוד [כבוד] as standing specifically, such as
 והנה שם כבוד ה' עמד (Ezek 3:23). This vision is described in Ezekiel in close proximity
 to the verse ממקומו ברוך כבוד ה'.

(87) It is also possible that these are two original parallel versions of the text,
 and not one version which developed from another (see further the beginning of §25
 as well as §26).

(88) The word מקום was well defined by Sharon (1996, 192): "the literal mean-
 ing of the word is 'the standing place of the feet'." He also pointed out the parallel
 between the development of the meaning "sacred place" and the development at-
 tested in Arabic مقام (ibid, pp. 191-193); see below, §24.

(89) The noun מושב has other referents, as well, such as "chair," as in the verse
 וישב המלך על מושבו כפעם בפעם אל מושב הקיר (1 Sam 20:25), and "sitting time," as in
 the verse ומושב בני ישראל אשר ישבו במצרים שלשים שנה וארבע מאות שנה (Exod 12:40).

(90) My friend Dr. Haim Cohen pointed out to me that this meaning of ישב also
 served as the basis for the halakhic conclusion that one must "reside" in a sukkah,
 based on the verse תשבו בנסוכה; the obligation is taken to be לישב בנסוכה, in which
 ישב means "to reside, to dwell" (compare, for example, Rabbenu Asher on b. Suk 46a [§3
 in chap. 4], as understood by his son, R. Yaakov Ba'al ha-Turim in Tur, אורח חיים,
 §643).

unleavened bread" (*Exod* 12:20), or ממושבתיכם תביאו לחם "You shall bring from your settlements bread..." (*Lev* 23:17). In all of these verses, when מקום appears it could be replaced with מושב, and when מושב appears it could be replaced with מקום, with no change of meaning. All of the verses deal with places – villages, towns, or any type of settlement – in which the Israelites dwelled permanently or temporarily.

§24 Moshe Sharon has already demonstrated that in both Hebrew and Arabic, the root קו"ם, from which מקום and its cognate مقام are derived, does not denote merely "to stand," but also "to stay, to reside, to dwell" (and when used with this meaning, it takes the preposition ב-). Sharon established this on the basis of a few verses, such as מי יעלה בהר ה' ומי יקום במקום קדשו "Who may ascend the mountain of the Lord, and who may stand in His holy place?" (*Ps* 24:3). In this verse, מקום קדשו (which could be paraphrased as מקומו) is the place in which he stays. (91) The verbal noun of the IV stem in Arabic from this root, إقامة, also denotes (as one of its meanings) "staying" or "dwelling" in a specific place.

§25 Our text adds an important piece of data to what was available from the verses discussed thus far: it provides evidence that its author believed that מקום could be replaced by מושב without altering the meaning of the sentence. One could go further and suggest that what we have is not a rewriting of the text, but two originally parallel versions, one reading ממקומו and the other reading מן מושבו.

It is clear that earlier מקום from קו"ם and מושב from יש"ב had different meanings. For the noun מקום, the primary meaning established was a general one: a free space for an object, or a person, or any other purpose (as mentioned, this has close semantic parallels in Aramaic אַתַּר, Arabic موضع, and 'place' in many European languages), whereas the noun מושב retained its etymological connection to the verbal root יש"ב, and its primary meaning is a seat, an area for sitting, or a time in which one sits or dwells in a land or city. But since both refer to areas, some speakers connected them. The identity of their meanings is reflected in the biblical verses quoted above (§23). The reference to מקומו of כבוד ה' (in *Ezekiel*), together with the reference to מושבו of כבוד ה' אלהים (in our text) provides conclusive proof that the two nouns had become synonymous.

§26 Obviously, when we speak of the two nouns as being synonymous, we mean a partial synonymity, and not a complete equation of the two resulting in their interchangeability in all contexts. As I already indicated, there is no way of proving that the author of our text did *not* replace מקומו with מושבו, and that instead we are dealing with two parallel versions of the text. It is clear, though, that even if this is true, the parallel versions demonstrate that the two words overlap se-

(91) See the incisive and detailed discussion of Sharon 1996, 193-195.

mantically. In either case, this is an important linguistic fact established on the basis of the *Vision of Gabriel*.

קט/קיטוט(*qt/qyṭwt*)

§27 In ll. 24-25, our text cites part of a biblical verse. What is cited is from *Haggai*: עוד מעט קיטוט היא ואני מרעיש את השמים ואת הארץ (2:6); in the text what is found is, עוד מעט קיטוט היא ואני מרעיש את השמים ואת הארץ (92). The blatant difference between the two texts is the phrase אחד מעט in *Haggai*, as opposed to קיטוט in the line in the text.

The first editors cautiously hinted at the possibility that one ought to connect the phrase קיטוט in our text to כמעט in *Ezekiel* (16:47): (qere: עשית (94) ולא בדרכיהן הלכת וכתועבותיהן עשיתי (94) כמעט קט (95) ותשחת מהן בכל דרכיך (96). The phrase is also found in a text published by Devorah Dimant (in which the last letter is missing): (97) והבא כמעט קט).

§28 It is indeed difficult not to see a clear connection between *Ezekiel*'s קט and קיטוט in our text. I will explain: both קט and קיטוט are *hapax legomena*. Regarding the word in *Ezekiel* there are differences of opinion about both its etymology and its meaning. Some connect it to Arabic فُط, "only, just." (98) The meaning of the phrase כמעט קט, then, is "just a bit (מעט)." Others have connected קט to *q^watit* in Ethiopic (Ge'ez and Amharic), meaning "small," (99) in which case קט מעט would mean "a small bit," (100) like the expres-

(92) The dots stand for two letters in the text which are erased or damaged.

(93) As mentioned above (§2), the original editors mentioned that this is a citation from *Haggai* (Yardeni and Elitzur, 156).

(94) Yardeni and Elitzur, 162.

(95) Those responsible for the *te'amim* placed the *etnaḥta* on the word עשיתי (so the Ketiv; Qere: עשית). This creates a major difficulty, for then the phrase כמעט קט is part of a clause which continues with a *waw*-consecutive form (ותשחת; if the phrase כמעט קט were connected to the verb השחית, it would be found as השחת). It is probable that the verse should be split at the word קט (see Ehrlich 1901, on the verse in *Ezekiel*, *ad loc.*; he believes that the break should come at the word כמעט, for in his view, "the word קט which appears afterwards and is never elsewhere attested in all of the Bible, is superfluous; it was originally written in the margin in order to explain [the word] כמעט, and to inform us that its meaning is that of בלבד in Mishnaic Hebrew"). See further below in the next paragraph.

(96) Yardeni and Elitzur (p. 162) deliberate over the meaning of the word קיטוט: "perhaps it is from the root קט, and if so, the meaning may be something like קטטה (quarrel) /riote and perhaps it is another version of קט מעט (*Ezek* 16:47)."

(97) See Dimant 2001, 42, and her notes on the line on pp. 44-45.

(98) Cf. Ben-Yehuda *Thesaurus*, and BDB, s.v. See also what was cited from Ehrlich above in n. 95.

(99) Cf. HALOT and Kaddari 2006, s.v. More specific data can be found in Leslau 1987, 455.

(100) It seems that these Isaian verses were in the mind of N. H. Tur-Sinai,

sion **כי עוד מעט מוער** found in a number of verses in *Isaiah*: **וכלה זעם** “For very soon My wrath will have spent itself” (10:25); **הלא עוד** “only a small remnant shall be left” (16:14); **ושאר מעט מוער** “surely, in just a little while, Lebanon will return to farm land” (29:17).

In practice, whether **קט** (101) is understood to mean “only, just,” or to mean “small,” it belongs to the semantic field of quantities, specifically small quantities. (102) Either way (whether we compare it to Arabic **قَط** or Ethiopic *qʷaṭit*, it is likely that the word is derived from a root **קט**“ט” (103) as **קטוט** transparently is. The vocalization of a noun derived from a geminate root with a *qames* instead of a *pataḥ* (**קט** → **קָט**) is apparently a result of its location in a second syntactic pause; it is therefore read with a *zaqef*. (104) In the Babylonian tradition, however, it is read **קָט**, (105) a form which could easily be the reflex of a geminate root, such as **חָץ**, **חֶץ**, and so on. (106)

§29 It seems to me that it is not at all difficult to explain the replacement of Ezekiel’s **קָט** by our text’s **קטוט**. A number of phenomena familiar to some extent within biblical Hebrew, and to a greater extent within Mishnaic Hebrew and later strata, co-occurred within the word **קטוט**. The following are the details.

- a. In Mishnaic Hebrew we find that the participles and adjectives derived from stative verbs interchange, such as occurs

when he wrote the entry for **קט** for Ben-Yehudah’s *Thesaurus*: it is defined there as **מעט מועיר**. Dimant (2001, 44-45) penned an important discussion of the connection between **קט** and the expressions **מעט מוער** (*Isa* 10:25, etc.) and **כמעט רגע** (*ibid.*, 26:20). See there, and see also the additional parallel cited by Dimant between the text of *Ezekiel* and *Pseudo-Clement*.

(101) Others have claimed that **קט** is a scribal error, an imprecise duplication of the final syllable in **כמעט** (see the view of Zimmerli cited in HALOT). Others have suggested other emendations, such as reading **קטן** or **קטן** (cf. Ben-Yehudah, n. 2 in the entry for **קט**, and BDB), as well as other corrections (see BDB).

(102) It should be emphasized that the three phrases – **מעט מוער**, **כמעט רגע**, and **כמעט קט** – are quantifiers, which emphatically limit the quantities of the object being quantified. Many of the earlier biblical interpreters understood thus, as well, and used the word **קט** in their own Hebrew writings: e.g., **בגדי בראותי זה הפרש** (Ibn Ezra); **וכמעט קט אני שותה בכוסך** (R. Shmuel ha-Nagid). These citations and others are quoted in Ben-Yehudah, s.v.

(103) Some have derived **קט** from the root **קט**“ט” (because of the vocalization **קָט**), which has the meaning “to cut.” This was the view of Radaq in *Sefer ha-shrשים* (as is noted in Ben-Yehudah, *Thesaurus*). R. Jonah ibn Janāh, on the other hand, derives the word from a biliteral root **קט**.

(104) Below in §§30-32 other examples are cited in which the [a] is transformed into an [ā] (**קָט** → **קָט**, **קָט** → **קָט**) under secondary stress. The emphatic consonant [q] could also induce a change of [a] to a back vowel [ā].

(105) See Yeivin 1985, 1069.

(106) The form **קָט** could also, of course, be explained as a reflex of a noun derived from a hollow root, as is the case with **נָר** and others. But this is not a difficulty, since analogical formations spread from geminate roots to hollow roots and vice versa often in the history of Hebrew.

with the pair קָצֵר/קָצָר. As is known these interchange with the *qāṭūl* pattern (the passive participle of the Qal), as well, yielding sets such as מָלִי/מָלָא, דָּבֹק/דָּבֵק, קָצֵר/קָצָר, קָצֹר/קָצָר (107)

- b. Alongside the *qāṭūl* pattern, it is known that there are at least three other verbal patterns which serve as passive participles in Hebrew: (1) *qāṭīl* (108) – the Hebrew cognate of Aramaic *qēṭīl* – such as קָרִיא alongside קָרֹא in BH, and שָׁלִיחַ alongside שְׁלֹחַ in MH; (109) (2) *qīṭṭūl*, such as לְמוֹד alongside לְמוֹד, שְׁלֹחַ alongside שְׁלֹחַ, and מְנוּי alongside מְנוּי; (110) (3) there is some evidence also for the use of the pattern *qittōl*, such as שִׁלּוּחַ alongside שְׁלֹחַ. (111)
- c. There is an increasing tendency in MH for many of the geminate verbs to be conjugated as if whole roots. For example, Biblical Hebrew שָׁח → MH שָׁחַח, בָּלֹתִי → בָּלְלֹתִי, and נָסַב → נָגַמַּס. (112)

§30 Thus is explained the interchange between verbs of the form *qatt* – the stative participle of geminate verbs – and verbs of the forms *qāṭūl*, *qāṭīl*, *qīṭṭūl*, and more, which are passive or stative participial forms from geminate verbs when conjugated like whole roots.

(107) See Sharvit 2006, 216-217; Sharvit 2004, 108; Bar-Asher 1983, §36, pp. 153-154. I very much like the brief formulation of Segal with regard to the use of the passive participles, which I quote with minor ellipses: “The passive participles [...] refer to a nearly stable situation which has been created as the result of an action which preceded the situation referred to by these participles, and they are for all intents and purposes truly adjectives. For example, *עַל הַחֹדֶשׁ* [one may not take *ma'aser sheni*] from the attached (מְחוּבֵּר) [grain] to fulfill the requirement of the detached (תָּלוּשׁ) [grain], and not from the new (חֹדֶשׁ) to fulfill the requirement of the old (יָשֵׁן) (*m. MS* 5:11); *לִיּוֹלָב הַגּוֹזֵל וְהִיבֵשׁ פָּסוּל* ‘a *lulav* which is stolen (גּוֹזֵל) or dry (יָבֵשׁ) is unfit’ (*m. Sul* 3:1) [...]. In all of these texts, the use of the passive participles (מְחוּבֵּר, תָּלוּשׁ, גּוֹזֵל) [...] is entirely equivalent to the use of the adjectives חֹדֶשׁ, יָשֵׁן, and יָבֵשׁ” (Segal 1936, 132). Indeed, this is the basis for the interchange of the participial forms and adjectives of stative verbs with the passive participles of the same verbs.

(108) The *qāṭīl* pattern has a geminated counterpart *qāṭīl*, as in pairs like חֲסִיד and צָדִיק. Attention should also be paid to the pair אֶבֶר/*אֶבֶיר (construct אֶבֶיר), both “mighty warrior.” The form אֶבֶר is used as an epithet or adjective for humans, e.g., אֶבֶר הָרָעִים (*I Sam* 21:8); סִלָּה כָּל אֶבֶירֵי אֲדָנִי בִקְרָבִי (*Lam* 1:15), whereas אֶבֶיר serves as an epithet or adjective for God, e.g., וְנֹאֲלַךְ אֶבֶיר יַעֲקֹב (*Isa* 49:28); הָאֲדָרֹךְ הַצְּבֹאֵר (*ibid.*, 1:14). On occasion the variation falls along the lines of different traditions, as I found to be the case with regard to חֲבִיב (see above, n. 56 and the reference cited there).

(109) Cf. Segal 1936, 91-92 (the data collected by Segal are valuable, but his own formulation of the explanation is outdated); see also Bar-Asher 2002b, §20, pp. 213-214.

(110) See Bar-Asher 1980, 14, 67-68, 113.

(111) See there, pp. 113-115.

(112) This was described well by Haneman 1980, 304-330 (Chapter 36: “Morphology of the geminates”).

§31 This type of interchange occurs more often in later periods.

a. **איגרתה לבוכים, ערוכים וזוכים, ו' מ' רופש פרוכים, ע' ככ** זכוף. (ha-Qillir, *Qedushtot* for Yom ha-Kippurim), (122) alongside the older form זוף אני, e.g.,

(114) The vocalization מְרִיר is not actually attested in the MSS, but is based on the reading proposed in the edition of *Ben Sira* of the Academy of the Hebrew Language.

(116) The same explanation was offered above (§28) for the form טָר in place of טַר .

(118) See the Academy's edition of *Ben Sira*, pp. 14 and 208 (there are doubtful variant readings recorded there, as well: מַמְרִיר). This form was cited already in Ben-Yehudah's *Thesaurus* s.v. מַרִיר, where other examples from later texts (the writings of Radaq and others) are also cited.

(120) It is cited this way by Ben-Yehudah, *Thesaurus*, s.v. מְרֹר (where another citation from *Pirqe de-Rabbi Eli'ezer* is also brought; see there). Still, R. 'Amr read מְרֹרִין. It seems very likely, however, that Ben-Yehudah correctly analyzed the form as מְרֹרִין, the plural of מְרֹר. This possibility is strengthened by the fact that the word is linguistically matched with three certain *qetūlīn* forms (מְרֹבֵּין, מְרֹבֵּין, and מְרֹבֵּין).

(121) This is the reading of R. 'Amr, following the Yemenite reading tradition, although in Aramaic he reads קליל, on the pattern *qatīl*: וְלֹא לִיעוֹל בְּךָ אִינִשׁ אַחֲרָיָא: דְּלֵא לִרְגִישׁ בְּךָ דְּקִלִּיל אַתְּ (b. *Git* 56b).

(122) The vocalization זְכוּכִים is shown by the internal rhyme with לְבוּכִים and פְּרוּכִים. The fact that the form appears to suit the needs of a rhyme does not detract from its independent value as evidence for the form זְכוּךְ.

שמן זית זך כתית למאור, בלי פשע (*Job* 33:8), *Exod* 27:20; *Lev* 24:2) – in this phrase, in both of its appearances, the word זך is at a secondary pause in the text (*teḥīr*), and therefore vocalized with an [ā] *qames* vowel. (123)

- b. Alongside צר (*Isa* 30:20) or צר (*Num* 22:26 [pause]), in medieval Hebrew the form צרור is also found, in the emphatic phrase צרה צרורה, attested in the language of R. Shemuel ha-Nagid. (124)
- c. The feminine form of תמים is also attested: שאלתה תמה ותמימה. מם...< חתומה. ומחותמה כחמה. ואני חומה, חכומה. מיום קרובות, *piyyuṭ* of Ḥaduta, ניתחכמה <תמומה (125) מכל אומה 16, l. 75). למשמרות... אלוה צרופה
- d. Alongside קל, also attested is קלול (apparently to be read צידוק הדין) וזה כמו החשוק אשר לא יעזוב: (קלול) in a Karaite text: (החושק... ואם יצא החשוק קלול ומקלה c. 1100). (126)
- e. The form דקיק (or דקיק) is already known in the language of Rabbenu Hananel (להגלי בהמה דקיקן אם הטילם לכבשן האש) (127) alongside the old form דק.

§32 The best of the parallels, and the pair which is most apt for our discussion is a pair already attested in the Bible: עז/עזו. (128) עז (which appears as עז under pause) appears in verses such as אפס כי עז ארור אפס כי עז, ארור אפס כי עז (*Gen* 49:7; here it is vocalized with an [ā] because of the secondary pause [*zaqef*] (129) on the word). (130) Alongside this form is attested its counterpart עזו וגבור: עזו (131) (*Ps* 24:8).

(123) See above, §28 and n. 116.

(124) Specifically, the line is: התיר ידי צרות צרורות (*Diwān ha-Nagid*, as cited in the lexicons: Ben-Yehudah does not cite the source, but Even-Shoshan's Dictionary cites the exact source); the phrase צרה צרורה then spread to be very common.

(125) This word was also needed by the *paytān* for the rhyme. The needs of rhyming is, of course, one of the reasons for the creation of new words, and for their appearances in texts.

(126) The work צידוק הדין was studied by G. Vajda in his article, "שני קיצורים," חקרי מזרח לזכר ד"צ בנעט in "של תאולוגיה קראית: ספר משיבת נפש ופרק צידוק הדין" (Jerusalem, 1979), 107-110 (my thanks to my friend and colleague Dr. Michael Ryzhik who brought this source to my attention). The reading קלול is presented in the *Ma'agarim* of the Historical Dictionary of the Hebrew Language of the Academy of the Hebrew Language.

(127) This is a citation from the commentary of Rabbenu Hananel on *b. Shab* 74b (cited in Ben-Yehudah, *Thesaurus*, where the form דקיק is vocalized [zaqef]; Ben-Yehudah also pointed out that the form is very common in Aramaic).

(128) My friend Dr. Haim Cohen, also mentioned this example to me.

(129) See above §28 and n. 116.

(130) In the six other instances in which the word appears as עז instead of עזו, the word falls on a verse's primary pause (*ethnaḥta* or the end of a verse), e.g., יתר עז (*Gen* 49:3; see also *Isa* 25:3; *Amos* 5:9).

(131) I will note that in the use of this word pair, biblical Hebrew seems to

In this light, the replacement of קט in *Ezekiel* with קיטוט in the *Vision of Gabriel* is easily explained. In my view, קיטוט is equivalent to קטוט, and equivalent to קטיט. The first of this triad, קיטוט, (132) is, in my opinion, the form attested in our text. The form קטוט, a form conjugated as if from a whole root, replaced the proper geminate form קט (קט), just as with regard to all the examples cited above, the form conjugated as a whole root developed alongside or as a replacement for the specifically geminate form. The pair קט (קט) – קטוט is an exact morphological parallel to the pair עז (עז) – עזוז attested in Biblical Hebrew. Fundamentally, however, it is parallel also to the pairs קליל – קל, מרור/מריר – מר, תמים – תם.

§33 To my mind, the cognates from Arabic and Ethiopic adduced by scholars and cited above (133) demonstrate that the word קט is not a textual corruption. Rather, the Hebrew words קט and קיטוט, Arabic كَطَط, and Ethiopic *qwaṭiṭ* are all derived from a root קט"ט. The only difficulty with the forms קט and קיטוט is the fact that they are attested so rarely, but they are certainly not the only *hapax legomena* in Hebrew which were in fact live forms within the spoken classical language, and the resolution of their derivations and meanings is well explained by reference to their parallels in other Semitic languages.

§34 Beyond that, the explanation is assisted at times by internal data. In other words, sometimes a *hapax* which is attested in a text of a certain era is clarified by a cognate attested in a text of the same language from a different era. Every linguist can supply examples of this type. I have recently shown this to be the case in a study of the word חנא (*Isa* 19:17), from חנ"ג, and the word מחיגה from the similar root חנ"ג (in the Qumran text 4Q374), and demonstrated that the two refer not just to "circular motion," but also to "confusion" and "bafflement," and perhaps even actual "insanity." (134)

This is the case, too, with regard to the two words which we have studied here: the word קט is clarified not only by its cognates in two other Semitic languages, but finds assistance also in its Hebrew cognate קיטוט, which is attested in a Hebrew text from a later stage of the language's history. As was mentioned, this text was composed and copied in late Second Temple times. Both קט and קיטוט occur af-

make an interesting distinction between sacred and profane. The normal form עז (or עז) appears modifying anything which is strong or powerful as it is – a nation, a person, an animal, a border, a characteristic. עזוז, on the other hand, is said about God, and refers to the most sacred of sanctities. This point will be spelled out in detail in another place (and there are, of course, other such instances as well).

(132) In the vocalization of the graphemic string קיטוט I prefer קטוט, in the *qitṭūl* pattern, which is the more common pattern, over קטוט, in the *qitṭōl* pattern, which is less common. It should be clear, however, that the vocalization remains still a (likely) guess.

(133) See above, §28.

(134) See Bar-Asher 2006a, 159-166, esp. 162-163 (= Bar-Asher 2008).

ter the word מעט in their respective sentences: (עשית qere: עוד מעט קיטוט היא ואנ ימרעיש (in one of *Ezekiel* visions), and מעט קיטוט היא ואנ ימרעיש (in the *Vision of Gabriel*). Each of them serves to emphasize or to quantify, adding weight to the word מעט, which is also a quantifier. In other words, the reference is not just to מעט "a little," but "a very little" (מעט מוער).

Summary and conclusions

§35 The initial publication of the inscription containing the *Vision of Gabriel* is an important accomplishment, and Dr. Ada Yardeni and Dr. Benjamin Elitzur deserve praise for a fine publication. The revised edition published by Prof. Israel Knohl also elucidates important details regarding the text of the inscription, (135) and allows for an investigation of the text's language – in addition to providing a thorough study of its contents. This text, like other ancient texts discovered in the past centuries, provide interesting linguistic data. It is true that because of the nature of the text, and because of the phrases which appear in it repeatedly – such as אמר אלהים צבאות (11), אמר (19-20), and a few more of this sort – this text contains less new facts than we may have hoped for; some of what is found, however, is fascinating. Each new datum adds to our knowledge of the history of the Hebrew language. In general, every epigraphic text contains details unparalleled in the rest of the corpus (the classical sources). The interpretation of some remains yet elusive.

§36 In summary, the contribution of the text is well represented by the details studied here. The basic findings are here summarized.

- a. Orthographic practices. The fundamentals of the practices studied are as follows. (1) The orthography (defective spellings) is generally comparable to that known from the Bible and the Bar Kokhba letters, as opposed to that known from the Qumran texts and Mishnaic Hebrew. The spelling of והשבין (= והסבין "and the elders") exemplifies the adherence to the biblical system; I assume that the word would have been spelled with a ש and not a ס in MH had it been attested there, as well, just as other biblical words are spelled in MH according to their biblical orthographies. (2) On the other hand, the text contains an increasing number of spellings of the masculine plural and dual endings, and masculine plural pronominal forms, with final נ *nun* instead of מ *mem*. (3) Also attested is plene spelling for [i], in קיטוט (= קטוט). The two

(135) Regarding at least two words, our own re-study of the text also provided new certainty to the readings שלושה in l. 28 [§16 n. 68] and הריב in l. 64 [§12 n. 55].

(136) See above, n. 52.

practices (2) and (3) tie this text to the practices seen in the manuscripts of rabbinic literature, primarily the manuscripts of the Mishnah and other Tannaitic literature (as well as to Aramaic).

- b. **Phonology and morphology.** I have tried to clarify a number of issues in these realms. (1) Regarding the gutturals, I have argued that there are some certain examples of their elision: the spelling **המלנך** in place of **המלאך**, which shows the non-pronunciation of the א, and **הביב** in place of **הביב**, which shows the non-pronunciation of the ה. It is possible, furthermore, that the spelling **אלי** in place of **אליה** (= **אֱלִיָּה**), which appears twice, testifies to the non-pronunciation of the ה. Finally, the spelling **האיה** = **הִיָּה** (or **הִיָּה**) may be attested in the text; if this reading is correct, it may reveal that the word was pronounced [eye] or [aye], without the ה being pronounced. (137) (2) We raised the possibility that the word **מלאכה** in the phrases **שלושה מלאכה** and **אל מלאכה** reflects the masculine plural ending [-e], as is found in the forms **חלונִי**, **אדירִי**, **מלאכִי** (!), (138) **שְׁמִנִי** (in the geographic noun **גַּת שְׁמִנִי**, a biform of **גַּת שְׁמִנִים**). (139) (3) Likewise, the text contains the masculine plural demonstrative pronoun **אל**, which is also found in Chronicles. (4) There are also forms which I have not studied in this paper, but will take the opportunity to mention here. The verb **תעמדו** is attested in l. 85 with the meaning “become alive” (about which Knohl had much to say) (140) is noteworthy, since Kutscher already noted that the inchoative (change-of-state) meaning of the verb **עמד** is a feature characteristic of the Hebrew of Second Temple times. (141) (5) Also worthy of mention is the word **סמן** (= **סִימָן**), apparently a loanword from Greek. (142)
- c. The detailed study of the two words **מושב** and **קטוט** also adds to our knowledge of the semantics and morphology of Hebrew. Regarding the first, the discussion focused on semantics alone, whereas for the second both the meaning and the morphology were at issue, as was the light which could be shed on the etymology and usage of the word **קט** which appears in *Ezekiel* (16:47).

(137) There may be one example of the non-pronunciation of the ע, as well: **לשות** (78, possibly ← **לעשות**), as hinted below in §39.

(138) See above, n. 75.

(139) We did not, however, reject Knohl’s suggestion that the form **מלאכה** reflects the 1st person plural possessive suffix.

(140) See above, n. 13.

(141) See Kutscher 1977, 400-401; he saw in this a reverse calque from Aramaic.

(142) See Albeck 1959, 210, and below, §39.

Clearly the significance of forms which are attested with certainty is far greater than that of forms whose very existence is debatable. Therefore the spellings more defective than the biblical spellings (such as **צבאת**), the plural ending **-ין** similar to that found in MH, and the readings **מִן מוֹשֶׁב** and **קִיטוּט** are more valuable than readings such as **הָיָה**, possible equivalent to **הָיָה**.

§37 In any event, many of the linguistic features enumerated here, especially when taken in conjunction, points to the end of Second Temple times. Especially diagnostic are the demonstrative pronoun **אֵל**, the relative particle **ש** instead of **אשר**, (143) the absence of the waw-consecutive forms, (144) the forms which end with **ו** (the masculine plural and dual forms, and the masculine plural suffix **-הן**), the use of the verb **עָמַד** with the meaning "become alive," the loanword **סמן** (= **סימן**), and more. However, there is also a connection to the language of the Bible; this is not limited to the frequent use of biblical expressions already noted by Yardeni and Elitzur, (145) but includes also some conservative linguistic features, of which the most notable is the defective orthography.

The convergence of the lines of evidence studied here adds support to the view that the text dates from the end of the Second Temple era. Since the paleography points to a date in the first century BCE, it is possible that the view of the first editors that the text was copied not long after its composition is to be accepted.

§38 The wider-ranging inquiries into the word-pairs **מוֹשֶׁב/מָקוֹם** and **קִיטוּט/קָט** presented here have independent linguistic significance, and are not to be directly tied to the dating of the text. The semantic equivalence between the words **מָקוֹם** and **מוֹשֶׁב** is attested already within the Bible (as exemplified by the parallel verses cited above, in §23). The text then provides more explicit evidence of this equivalence, but cannot be used to date the text.

The examination of the pair **קִיטוּט/קָט** also yields semantic and morphological information which retains independent value irrespective of the date of the data. It is true that the interchange within geminate roots between the forms **פָּע** and the participles and adjectives conjugated according to the rules of whole roots (**פָּעוּל**, **פָּעִיל**, **פָּעוּל**, and more) becomes more common in Mishnaic Hebrew and later dialects, but they are attested within BH, as well (such as **תָּם/תָּמִים**, and especially **עוֹז/עוֹזִים**).

It is nearly superfluous to add that not every linguistic detail in a given text has to be tied to the question of the text's date.

(143) See above, §4 n. 16.

(144) See Yardeni and Elitzur, p. 156.

(145) See above, §4 and n. 17.

In the wake of the study

§39 In the wake of this study I would like to add that there are further linguistic details in the text which are worthy of detailed study, and they will have their day. One example of this type is the noun phrase **שסמן מירושלם** (36). I already mentioned that the first component in this phrase is the noun **סִמָּן**, which is a loanword from Greek. In this text this serves as a synonym for the noun **אוֹת**, which appears a number of times in the text, as well: for example, **אותגלות** (37). (The edition of Yardeni and Elitzur indicates that the two words of the latter phrase are written together like one word, although Knohl reads a space between them: **ואות גלות**.) Our text is apparently the earliest attestation of the noun **סִמָּן** within Hebrew, but the syntax of the noun phrase still demands further study.

There are also a few words which are not transparent, such as the word **שאלני** in the clause **אתה שאלני** (146) **ידוד** (11). Is this perhaps the participle **שואל** to which the 1cs suffix **ני** instead of **י** was added? There are also a large number of words which are not sufficient understood because of the state of the text's preservation. For example, it is not clear what the word **לשות** (78) is: is this the infinitive **לעשות** with loss of the **ע**? The text is so damaged that it seems that any speculations would be futile at this point. (147)

Additional note to §§11-15

§40 While making final corrections to this article, I learned that advanced technologies were being employed in an effort to decipher the text as fully as possible, especially with regard to the doubtful and controversial readings. In conversations with a number of people, the possibility arose that two of the readings discussed above may have to be discarded, namely, **הביב** in l. 64 and **האיה** in l. 80.

If indeed it turns out that the reading is to be **חביב** with a **ח** and not **הביב** with a **ה**, and that there is another word to be read in place of **האיה**, the upshot will be that two of the words we relied on as evi-

(146) See above, n. 52.

(147) I will give one example of the type of speculation which is not worth entertaining at this point. In l. 64, Yardeni and Elitzur read,]... ל. [] ה/חביב.. ל. []. Very similar is the reading given by Knohl, except that he does not indicate the presence of the letter **ה** at the end of the word prior to the word **החביב** (cf. above, §12). I already indicated above that the correct reading should be **הביב**, with a **ה**. Should we restore the word **עם** in the lacuna before **הביב**, and read **הביב (=חביב)**? This phrase, as Menahem Kister showed, is a reflex of the Aramaic Targum of the Hebrew phrase **עם סגולה** (cf. *Onqelos to Deut* 7:6; 14:2; 26:18), and it was encountered in *4Q462* I 11: **עם החביב יעקנוב** (cf. Kister 2000, 137; Bar-Asher 2002a, 17-19). It is obvious that to justify such a restoration a wider context is needed, and this is lacking within l. 64 and the line preceding it. All this, then, is much speculation resting on very little evidence, and worth very little.

dence for the weakening of the pronunciation of the η will have to be eliminated from the discussion (and, in fact, we were very careful in our presentation above to always make clear the tenuous nature of the evidence). If this is the case, the evidence remaining in our hands will be the reading $\eta\eta\eta$ for $\eta\eta\eta$ and $\eta\eta$ (2x) for $\eta\eta$. In other words: we will still have the requisite evidence to demonstrate the loss of the two laryngeal consonants, η and η .

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Moshe BAR-ASHER

A. "Vision of Gabriel" in the edition of Yardeni and Elitzur

(doubtful readings appear in underlined font)

טור ב	טור א
[45]	1. []
[46]	2. []
[47]	3. []
[48]	4. []
[49]	5. []
[50]	6. []
[51]	7. []
[52]	8. []
[53]	9. []
[54]	10. []
[55]	11. []
[56]	12. []
[57]	13. []
[58]	14. []
[59]	15. []
[60]	16. []
[61]	17. []
[62]	18. []
[63]	19. []
[64]	20. []
[65]	21. []
[66]	22. []
[67]	23. []
[68]	24. []
[69]	25. []
[70]	26. []
[71]	27. []
[72]	28. []
[73]	29. []
[74]	30. []
[75]	31. []
[76]	32. []
[77]	33. []
[78]	34. []
[79]	35. []
[80]	36. []
[81]	37. []
[82]	38. []
[83]	39. []
[84]	40. []
[85]	41. []
[86]	42. []
[87]	43. []
	44. []

B. "Vision of Gabriel" according to the edition of Israel Knohl

"This edition is based on inspection of the text itself, plus the photographs of the text. This inspection revealed that the edition of Elitzur and Yardeni is generally exact and reliable. The spots in which a new reading is proposed are marked by footnotes; letters which are illegible are indicated by a period; letters which are broken or otherwise doubtful are underlined." (148)

טור א שורות: 11-42

- 11 יהוה אתה שאלני כן אמר אלהים צבאות (149) [?]
- 12 [] ..גני מביתי ישראל ואגדה בגדלות ירושלם
- 13 [] אמר יהוה אלהי ישראל הנה כול הגאים
- 14 ... יח [נו] על ירושלם ר [] ומתוכ [ה] מוגלים
- 15 אחת שתין שלוש ארבעין נביאין והשבין
- 16 [ו]החסידין עבדי דוד בקש מן לפני אפרים
- 17 [ו]שים האות אני מבקש מן לפניך כי אמר
- 18 יהוה צבאות אלהי ישראל גני מבוכרים
- 19 קדשה לישראל לשלשת ימין תדע כי אמר
- 20 יהוה אלהים צבאות אלהי ישראל נשבר הרע
- 21 מלפני הצדק שאלני ואגיד לכה מה הצמח
- 22 הרע הזה לובנסד אתה עומד המלאך הוא
- 23 בסמכך אל תירה ברוך כבוד יהוה אלהים מן
- 24 מושבו עוד מעט קטוט היא ואני מרעיש את
- 25 .. השמים ואת הארץ הנה כבוד יהוה אלהים
- 26 צבאות אלהי ישראל אלה המרכבות שבע
- 27 [ע]ל שער ירושלם ושערי יהודה ינחו למען
- 28 שלושה (150) מלאכה מיכאל ולכול האחרין בקשו
- 29 אילכם כן אמר יהוה אלהים צבאות אלהי
- 30 ישראל אחד שנין שלושה ארבעה חמשה ששה
- 31 [שב]עה אל מלאכה מה זו אמר הצץ
- 32 השני [] ..ל.ד.פכ..... ואלוף השני
- 33 שמר על ירושלם שלושה בגדלות
- 34 [] ..והו(?)..ד. שלושה [] והו.ד.
- 35 [] ...[] . שראה איש ...עובד
- 36 שהוא שבו. [] שסמן מירושלם
- 37 אני על.איי. [?] אפר ואות גלות ..
- 38 [א]ות גלות. צל י.ל אלהים עין אנ. וראו
- 39 ג.....א... [] ירושלם אמר יהוה
- 40א.ל. למלא טחבו רוב
- 41 [] דם שירם הצפוני
- 42 [] דראון הננע בכול

(148) This paragraph is Knohl's introduction to his edition. Most of the apparatus included by Knohl to buttress and explain his readings has been omitted here.

(149) Knohl omitted ll. 1-10 and 43-44 in col. 1.

(150) Knohl read שלושה, whereas in my view the text reads שלושת.

טור ב שורות 54 – 87

- 54 [] שלשת ימין זה שאמ... [] הוא(151)
- 55 אלה [] ..[] של... [] ..[]
- 56 ראו נא הצפונין חו [נה] []
- 57 סתום דם טבחי ירושלם כי אמר יהוה צבאות
- 58 אלהי ישראל כן אמר יהוה צבאות אלהי
- 59 ישראל מא.. ל... אל... [] ..[] ד.. [?]
- 60 ה.לני.ך יחמול. רחמן קרב [ין] ..[]
- 61 .. [] ל אשריא..... תץ ש ... [?]
- 62 בת.ל א. ..ע.נ []
- 63 א. [] אב. [?].א. [] ...[] ..[]
- 64 [] ..[] ח/חביב...ל []
- 65 שלושה קדושי העולם מן מק. []
- 66 [] ו.שלוש אמר עליך אנחנו בטוחין []
- 67 בשר לו על דם זו המרכבה שלהן ..ל. []
- 68 אוהבין רבים ליהוה צבאת אל>ה<י ישראל
- 69 כה אמר יהוה צבאת אלהי ישראל מ.....
- 70 נביאים שלחתי אל עמי שלושה רועי אומר
- 71 שראיתי ברכ. ל..לך דבר. בר [] ..ב.. [?]
- 72 המקום למען דוד עבד יהוה [] ..א. [] ..[]
- 73 את השמים ואת הארץ ברוך ש..... []
- 74 אנשים עושה חסד לאלפים מ..... חסד. []
- 75 שלושה רועין יצאו לישראל.ל... [] ...[]
- 76 אם יש כהן אם יש בני קדושים ...ה.. []
- 77 מי אנכי אני גבריאלי המל.כי.לי..מל []
- 78 תצילם נביים גר..ם לשנות. [] ..ב..[]
- 79 מלפניך שלושה הא [ת]ות שלושה ... אק []
- 80 לשלושת ימין חאיה אני גבריאלי גו[ר] על[ן]
- 81 שר השרין דומן ארובות צרים א [] ..א. [?]
- 82 למראות ה...לשנם מ [] ..[] וה.ב.ג.מ. [?]
- 83 לי מן שלושה הקטן שלקחתני אני גבריאלי
- 84 יהוה צבאת אלה [ן] יש... []
- 85 אז תעמדו א. [] ל [] ..א. [?]
- 86 יל. א. \
- 87 ב.... עלם \

A FORGOTTEN QUMRAN CAVE 4 DEUTERONOMY FRAGMENT (4Q38D = 4QDEUT^U)

Summary

PAM 43.011 contains an unpublished Cave 4 *Deuteronomy* fragment, preserving parts of *Deut* 24:20-22, that cannot be assigned to an already published *Deuteronomy* manuscript.

A FEW years ago, when I went systematically through the PAM photographs, searching for “forgotten” fragments, I found an overlooked fragment in PAM 43.011. This photograph of Museum Plate #1076 shows 4Q14 (4QExod^c) frags. 35, 36, 38, and 40–45, all of which published by Judith Sanderson, but also a different, unpublished fragment in the upper left of the plate, containing remnants of *Deut* 24:20–22. To my knowledge, no-one has referred to the presence of this fragment on this plate, nor, for that matter, to the identification with *Deuteronomy*. (1) In the series of PAM photographs, the fragment first seems to have been photographed in PAM 42.004, a photograph with mainly Pentateuchal fragments, and subsequently on an earlier photograph of Plate #1076 with the 4Q14 fragments, PAM 42.739. (2)

This fragment, of which I only studied images, has a maximum height of 5.3 cm, and a maximum width of 6.3 cm. (3) However, the

(1) The presence of this fragment on the ROC 1076 Museum Plate with 4Q14 fragments has not been recorded by either Judith E. Sanderson, “14. 4QExod^c,” in *Qumran Cave 4 VII Genesis to Numbers* (DJD XII; Oxford: Clarendon Press, 1994), or Stephen A. Reed et al., *The Dead Sea Scrolls Catalogue: Documents, Photographs and Museum Inventory Numbers* (SBL Resources for Biblical Study 32; Atlanta, Ga.: Scholars Press, 1994).

(2) I thank the Israel Antiquities Authority for providing a digital photograph of PAM 42.739, and for the permission to publish the photograph.

(3) Study, measurements, and transcription of the fragment are based on PAM 42.739, from the CD-ROM *The Dead Sea Scrolls Electronic Library Volume 1*

The fragment may be transcribed as follows:

(6) Those are the two manuscripts described in Frank Moore Cross, Jr., "The Development of the Jewish Scripts," in *The Bible and the Ancient Near East: Essays in Honor of William Foxwell Albright* (ed. G. Ernest Wright; Garden City, N.Y.: Doubleday, 1965), 133–202, at 138 fig. 2 lines 1 and 2, and 166–73 where *4QDeut^a* is

teristic are the narrow *reš*, the high left arm of *tet*, the narrow forms and rounded bases of medial *kap* and *mem* (though in line 3 the base of *kap* in כ is angular), or the curved base of the relatively narrow *taw*. On the other hand, the height of the letters already tends to become the same. Comparison with all the other Cave 4 *Deuteronomy* as well as the Cave 4 *Reworked Pentateuch* manuscripts does not show a clear correspondence of hand, which presumably was the reason why the fragment was placed separately on another Museum plate.

How should we number and call this fragment? In *DJD XIV*, various editors published 4Q28-4Q44 as 4QDeut^a through 4QDeut^g. Instead of one number 4Q38 for 4QDeut^k, Julie Ann Duncan used the numbers 4Q38, 4Q38a and 4Q38b for respectively 4QDeut^{k1}, 4QDeut^{k2}, and 4QDeut^{k3}. In *DJD IX*, we find two *Deuteronomy* manuscripts written in palaeoHebrew, namely 4Q45 and 4Q46, with the sigla 4QpaleoDeut^r and 4QpaleoDeut^s. (7) In 2001, Émile Puech published two *Deuteronomy* fragments which he numbered 4Q38c (4QDeut^r). (8) However, since 4Q45 already has been called 4QpaleoDeut^r, it would be better to call Puech's new *Deuteronomy* manuscript 4QDeut^t. It is not clear what system of numbering one should use for newly identified manuscripts, and therefore I provisionally suggest to number on where Puech left, and call this fragment 4Q38d (4QDeut^u). (9)

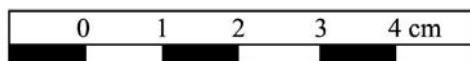
Eibert TIGCHELAAR

his example for the transitional period (ca. 175–150 B.C.E.), and 4QDeut^c for a typical Hasmonaean script (ca. 125–100 B.C.E.); cf. also, similarly, his "Palaeography and the Dead Sea Scrolls," in *The Dead Sea Scrolls after Fifty Years: A Comprehensive Assessment Vol. I* (ed. Peter W. Flint and James C. VanderKam; Leiden: Brill, 1998), 379–402, and Plate 10 lines 1 and 2.

(7) Patrick W. Skehan et al., *Qumran Cave 4 IV Palaeo-Hebrew and Greek Biblical Manuscripts* (DJD IX; Oxford: Clarendon Press, 1992), 131–54.

(8) Émile Puech, "Identification de nouveaux manuscrits bibliques: *Deutéronome et Proverbes* dans les débris de la Grotte 4," *RevQ* 20/77 (2001): 121–27, Pl. 1–3; cf. my discussion in "Minuscule Qumranica I," *RevQ* 21/84 (2004): 643–48, at 647. For the identification of three more *Deuteronomy* fragments as belonging to 4Q29 and 4Q34 see *ibidem*, at 646.

(9) In due time, an updated authoritative list of manuscripts, with specification of numbers and names, should be published.



PAM 42.793 (*4Q38d* = *4QDeut^a*)

A NOTE ON *11QPsalms*^D FRAGMENT 1

IN *DJD* 23, Florentino García-Martínez and Eibert Tigchelaar published a “clay object” that contained 14 recognizable Hebrew letters, in mirror writing. (1) They determined that the writing preserved parts of verses 2-4 of *Psalm* 6, and represented a fragment of *11QPsalms*^d. García-Martínez and Tigchelaar labeled this text as fragment 1 of *11QPsalms*^d and they read and reconstructed it as follows: (2)

1. [ואל בהמתכה] תיסרני חונני כי אמלל אני רפאני יהוה כי נבהלתי
2. [עצמי ונפשי נבהלה] מואדה] (3)

García-Martínez and Tigchelaar compared this fragment, appropriately, to three similar script-bearing marl blocks found in Qumran Cave 7. The 7Q fragments contain Greek letters in mirror script. (4) On the largest of the three marl objects, 34 letters can be discerned; on each of the smaller blocks, only four letters can be read. Cave 7 was discovered by Roland de Vaux and was excavated in proper scientific fashion. It is my understanding that the traces of letters upon these three lumps of marl indicate that Cave 7 disintegrated after

(1) F. García Martínez, E.J.C. Tigchelaar, and A.S. van der Woude, *Qumran Cave 11 II, 11Q2-18, 11Q20-31* (*DJD* 23; Oxford: Clarendon, 1998), 63-64 and 66, pl. 7.

(2) The second word in the first line appears in *DJD* as ‘בהמתכה’ rather than ‘בהמתכה’, and יהיה appears rather than the Tetragrammaton in the reconstruction, but this are undoubtedly typographical errors. Moreover, MT reads חנני יהוה כי אמלל; In *DJD*, the text is reconstructed as חונני כי אמלל, without the Tetragrammaton. The apparatus lists the variant plene spelling of חונני, but does not mention the Tetragrammaton at all.

(3) “1. [and do not in your fury] chastise me³. Have mercy [on me, for I languish; heal me, O Lord, for shaken 2. with terror are my bones, and my soul is] ⁴very [shaken with terror...]”.

(4) M. Baillet, J.T. Milik and R. de Vaux, *Les ‘Petites Grottes’ de Qumrân* (*DJD* 3; Oxford: Clarendon, 1962), 145-146, pl. 30.

heavy rains, and when the marl blocks fell onto the floor of the cave, they were saturated with water, so that the ink was transferred to the marl from scrolls that were on the floor of the cave. In contrast to the marl objects from Cave 7, which preserve remnants of letters transferred from papyrus scrolls, the letters upon the Marl object published by García-Martínez and Tigchelaar in *DJD* 23, were from a leather scroll.

García-Martínez and Tigchelaar noted that the distance between the lines preserved on the marl object is wider than the interlinear spaces found on the 16 surviving leather fragments of *11QPsalms*^d. Also, none of the 16 surviving fragments features the lengthened orthography of the word מואדה attested on the marl. A close examination of the photograph shows that the handwriting preserved on the marl object is different from the handwriting on the surviving leather fragments of *11QPsalms*^d. (Note, especially, the triangular *samekh* on the marl, in contrast to the squared *samekh* on the leather fragments.) It thus seems improbable that *11QPsalms*^d is the source of the letters preserved on this marl object.

The editors noted that all of the details known to them about this marl object were derived from a single photograph, PAM 44.012. The comment attached to this photo, in the catalog of the negatives, reads: "11Q? one frag; seemingly reverse image on stone or plaster." (5)

It is difficult to suppose that this marl object was in fact discovered in Cave 11. This cave is a natural cave found in the limestone cliff, and not an artificial cave dug into the marl terrace. It is quite certain that Cave 11 had always remained dry, since, if any substantial moisture had penetrated to cave 11, then the four relatively complete scrolls and 27 partial scrolls found in this cave would not have survived. It thus appears that the marl object containing the 11 Hebrew letters in reverse script would have been found in one of the collapsed marl caves near Khirbet Qumran, and not in Cave 11. Since the catalog contains a question mark next to the notation attributing this marl object to Cave 11, we may suppose that this piece of marl was discovered by the Bedouin, and reached the Rockefeller Museum after it was purchased by Iskander Shahin (Kando), presumably together with fragments of scrolls from Cave 11. It is most likely that this object was discovered by Bedouin in one of the collapsed caves in the marl terrace near Khirbet Qumran. It is unlikely that it was

(5) S.A. Reed, *The Dead Sea Scrolls on Microfiche: Inventory list of photographs* (Leiden, New York and Köln: Brill, 1993), 94. Unfortunately, there is no record of the date on which this photo was taken. According to the number of the photo, it seems that it was taken at a relatively late date. It could be that the fact that the marl object reached the Rockefeller Museum at a relatively late stage led to the supposition that it had been found in Cave 11. See E. Tov, *The Dead Sea Scrolls on Microfiche: Companion Volume* (Leiden, New York and Köln: Brill, 1993), 92.

found in Cave 4a or Cave 4b, since neither of those caves had collapsed. We should not dismiss the possibility that the marl object had been stolen by one of the workers excavating in the collapsed marl caves discovered by Roland de Vaux (that is, Caves 8, 9, or 10). This supposition is somewhat difficult, however, since we would expect that workers would have preferred to take fragments of scrolls rather than a lump of marl with remains of ink. (6) It thus seems most likely that this marl object was discovered in one of the collapsed marl caves near Khirbet Qumran that were excavated by the Bedouin. (7)

On the basis of this hypothesis, we posit that the letters remaining on this piece of marl comprise the sole surviving remnant of a scroll that had once been in one of the collapsed marl caves near Khirbet Qumran. It seems to me that the two and a half legible words on this marl object do not provide a sufficient basis for identifying the composition that was written upon the scroll that the piece of marl fell onto, though I do not reject the possibility that they may comprise remnants of *Psalms* 6

Hanan ESHEL

(6) On the supposition that a small fragment from 8QGen was stolen in the course of excavations in cave 8, see E. Eshel and H. Eshel, "New Fragments from Qumran: 4QGen^f, 4QIsa^b, 4Q226, 8QGen and XQpapEnoch," *DSD* 12 (2005): 134-157.

(7) On artificial caves in the marl terrace that collapsed over the course of the years, that were excavated by the Bedouin in the 1950's and early 1960's, see M. Broshi and H. Eshel, "Residential Caves at Qumran," *DSD* 6 (1999): 328-348.

DECONSTRUCTING THE SO-CALLED *GENESIS APOCRYPHON* FROM MASADA (*Mas Im* or *MasapocrGen*)

A CLOSE revision of photo 302362 of the fragments (1) published by Shemaryahu Talmon (2) as “Genesis Apocryphon” (3) has led the present author to the conviction: firstly that the most substantial fragment (1) has been written by two scribes (A and B); and secondly that two other fragments have been written by yet another scribe (C). The fragments should therefore rather be regarded as (at least) two different scrolls whose genre and theme cannot be identified due to their highly fragmentary condition.

On February 2, 1964, Yigael Yadin and his team excavating Masada discovered fifteen fragments in casemate, locus 1045. (4) Yadin tentatively classified this find as Apocryphon of Esther. (5)

(1) I gratefully acknowledge the help of the Israel Antiquities Authority who kindly provided the author with a high quality scan of the photo 302362 (plate on p. 542).

(2) S. Talmon, *Masada VI. The Yigael Yadin Excavations 1963-1965. Final Report. Hebrew Fragments from Masada* (Jerusalem, 1999), 98-104. Earlier versions in *EI* 20 (1989) 278-286 [in Hebrew] (*non vidi*) and *IEJ* 46 (1996) 248-255.

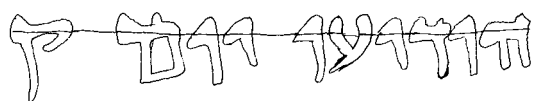
(3) The composition should of course not be confused with the *Genesis Apocryphon* from Qumran Cave 1: *1QapGen+1Q20*+ms Schøyen 1926/4c.

(4) Yadin writes about the archaeological context: “The whole of casemate 1045 was covered by a thick layer of ashes, especially in its southern third, where the ash layer reached a height of 2 m (!). Among the remains of the huge conflagration, traces of which could also be observed on the wall plaster, were found many pieces of burnt furniture, metal vessels and strips of embroidered cloth. There can be no doubt that furniture and utensils were collected from this vicinity (from locus 1042) and brought to this room to be deliberately incinerated. Many coins from the period of the Revolt were found in this conflagration layer, including a silver sheqel of the Year Five ... Also noteworthy are several scroll fragments found at the northern end of this room below the fallen column drums, as well as several ostraca with Hebrew letters and rows of numerals.” (*IEJ* 15, 76).

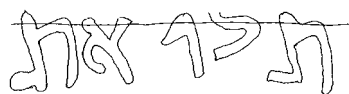
(5) “A few fragments were found of a scroll written in a formal script of the

After Yadin's untimely death, the board dealing with his legacy from Masada asked Shemaryahu Talmon to take over the task of publishing these and other unpublished Masada fragments. Talmon published the fifteen fragments from locus 1045, which he found arranged on one plate, as one scroll, *Mas 1m*, tentatively identifying it as a *Genesis Apocryphon*. Yet it is unclear whether Yadin believed that *all* fragments from locus 1045 should be associated with the "Apocryphon of Esther." In Qumran we do have many examples of scroll fragments being found in one cave and assembled on one plate but belonging to different compositions. Furthermore, Yadin seems to have arranged the fragments on the plate in six subgroups suggesting he was not convinced that all fragments belonged to one work. In a conservative fashion, Talmon's numbering still preserves the arrangement of the fragments: 1, 2a-f, 3a-b, 4a-d, 5, 6, (6) on which he notes: "The fragments are kept under glass covers arranged in six subgroups, *presumably* at the instruction of Yadin who was *possibly* guided by the similarity of edge contours." (7) While the first assumption, that Yadin is responsible for the ordering is probably true, the second presumption is unfounded as the edge contours do not resemble each other. The reason for the division has to be different: the handwriting and, perhaps, the character of the leather.

Seven fragments of *Mas 1m* have enough data to serve as sound basis for a detailed paleographical analysis: 1, 2a, 3a, 4a, 4d, 5 and 6. (8) Despite the fact that most of these fragments are relatively small with often just a few visible letters, the script on the fragments differs to that extent that it is impossible to argue that they all belong to the same hand. The two columns of fragment 1 have clearly been written by two different scribes as form, slanting angle and size of the letters betray. Finally, the scribe of col. ii hangs his letters a little higher on the rule than the scribe of col. i. The difference is most easily perceivable when looking on complete words:



Scribe B (fig. 1 ii)



Scribe A (fig. 1 i)

advanced Herodian style. We have not been able thus far to identify the text. From the combinations תן בריני and תלו את it seems to be a kind of apocryphon on Esther, but the other phrases do not allow to draw any clear conclusions in this matter." (Yadin, *IEJ* 15, 105).

(6) Labels for frags. 1, 2f and 6 are curiously absent on illustration 12 in *Masada* VI, 99.

(7) *Masada* VI, 100 (my emphasis).

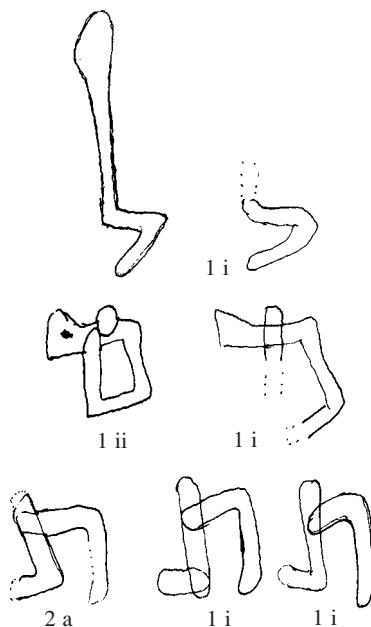
(8) Three of these fragments bear no traces of writing (2c, 2d, 3b) and five more have only very fragmentary remains of letters (2b, 2e, 2f, 4b-c).

There are very few letter forms attested in both columns and most of them are fragmentary. Besides the not very characteristic *waw*, all of them differ clearly from those on column ii:

a) *lamed*: the “belly” is rounder on col. i and the diagonal line is a little longer.

b) Final *mem* is missing the characteristic independent tick in the middle of the roof. Instead the left downstroke begins slightly above the roof. It is much longer than the almost square *mem* from col. ii. The lower left corner is further down and the lower right corner is further right, therefore the angles of the right downstroke and the bottom differ considerably from the *mem* of col. ii. The serif is made in a different fashion.

c) *Taw*, while not attested on 1 ii appears on frg. 2a whose letters are very similar to frg. 1 ii. (9) On frg. 2a, the letter is broader than on frg. 1 i, the right shoulder is not as high and the left downstroke is more diagonal.



As a general tendency, the downstrokes of *waw* and *taw* of column I are a little convex while the downstrokes of the letters extant on column ii are rather straight. (10) All strokes in col. i have the same width. In col. ii, horizontal strokes tend to be thicker than vertical ones and as a result letters look thicker at their top than at their base making the script very beautiful. Letters on column i are usually considerably larger. (11) Most probably therefore, col. i has been written by one scribe (A), col. ii by another scribe (B) who is also responsible for frg. 2a and probably also the other small fragments with the exception of frgs. 3a and 4d to be discussed now.

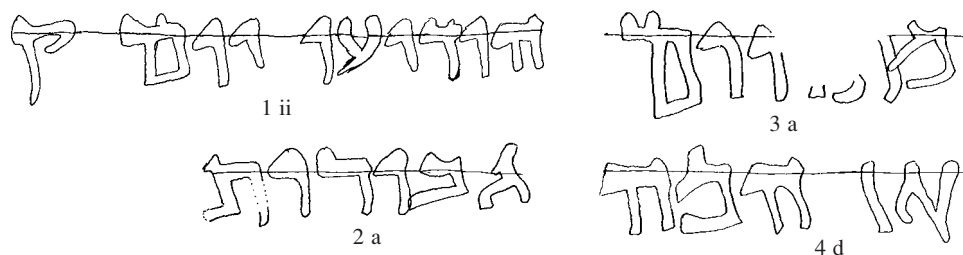
(9) However, the left leg of *gimel* of frg. 2a and 4a meets the axis at a slightly lower point and the whole letter is broader. In addition it seems to be topped by an extra stroke. The *gimel* of frg. 1 looks more symmetrical. *Lamed* on frg. 2a has a broadening at the very top only, while frg. 1 draws the broadening further down. Furthermore, on frg. 2a, its vertical line is slightly concave and the left downstroke is shorter.

(10) Of course, we cannot exclude that some but not all differences might have been caused by the fire (and later gelatinization) that attack the scroll from different sides and may have influenced the shape of the leather in various ways (look at the bottom of frg. 1).

(11) Roughly 65-70 pixels versus 50-55 pixels on my 600 dpi scan.

Regardless as to whether the distinction of two hands on frg. 1 is accepted, the writing of fragments 3a (and 4d) (12) differs clearly from both hands of fragment 1 and all other fragments of *Mas 1m*, especially the other big fragment 2a. This can be seen especially in writing angle, the forms of *ayin*, and final *mem* but also *waw* / *yod* and *aleph*. (13) The size of letters and the interlinear space differs, too, but this is less indicative.

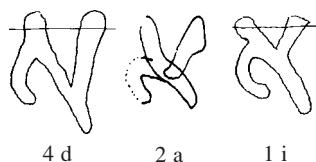
Again, the clearest impression can be gained from a comparison of complete words hanging from the rule (for scribe A see above):



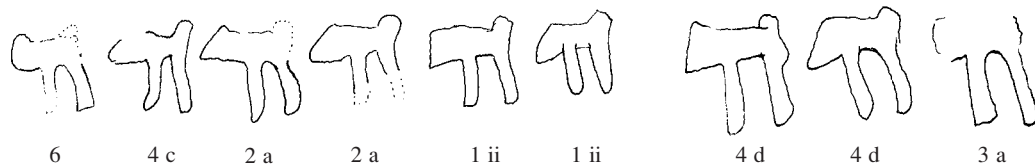
Scribe B (frgs 1 ii and 2a)

Scribe C (frgs 3a and 4d).

If we look on each letter, the *aleph* on frg. 4d differs from both frg. 2a (scribe B) and frg. 1 i (scribe A), not only in size (being even larger than the *aleph* from frg. 1) but also in form. The right downstroke meets the axis considerably further down. The left downstroke meets the axis further up.



He differs in size and it is also written slanting more to the left.



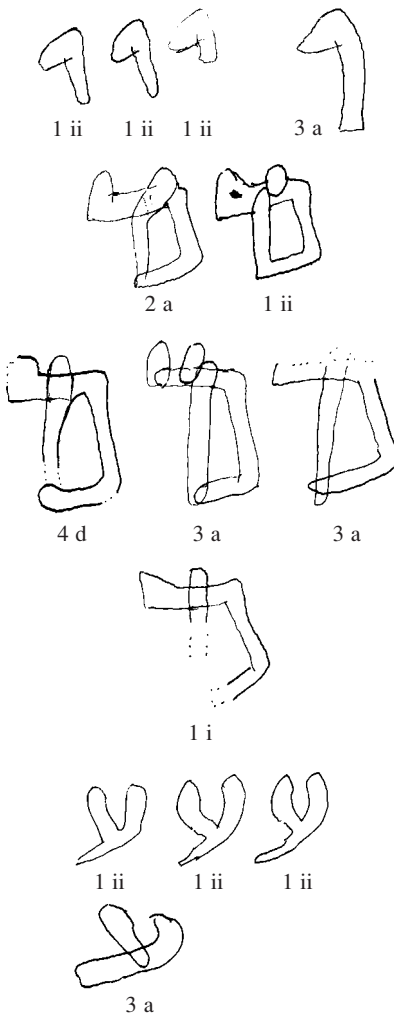
(12) Talmon's statement that frg. 3a is the only one not to portray dry (horizontal) ruling (*Masada VI*, 101) is mistaken – this fragment does have rules. We have too little data to be sure that 3a and 4d have been written by the same hand, but the size of letters and the slanting angle is very similar. In frg. 4d horizontal strokes seem thicker and vertical strokes thinner than in frg. 3a. The left side of the roof of *he* seems to go further down. Medial *mem* differs slightly, being rounder in frg. 3a.

(13) Talmon himself seems to have become more aware of this problem during the different stages of publishing these fragments. A rather swerving sentence "Recurring letters are identically executed." is continued by the observation: "An exception is the rather large tet (1350/4d), and the somewhat irregular lettering on frg. 1350/3." The comment on the *tet* is still missing in Talmon's *IEJ* article.

A letter of frg. 3 read by Talmon as *yod* differs completely from *yod* on the other fragments. If we read it as a *waw*, the downstroke of *waw* on the other fragments is not convex as this one (see drawing above).

Final *mem* attested twice on frg. 3a and once on frg. 4d (this is not a medial *mem* as wrongly stated in *Masada* VI) is much longer and has a distinct serif instead of a characteristic thickening. The tick in the middle of the roof attested in final *mem* of scribe B is absent. The letter is much longer than the almost perfect square form of scribe B. Fragments 3a and 4d also differ from scribe A. The final *mem* is longer and narrower. The right downstroke is not concave and there is an extra-tick on the roof slightly to the left of the beginning of the left downstroke. Scribe A causes a similar effect by beginning the left downstroke a little higher than the roof and continues straight down in one single line. Unlike the *mem* of scribe A, the *mem* of frg. 3a and 4d has a small separate downstroke as serif. (14)

On fragment 3a, *ayin* lies more to the right (ca. 18° vs ca. 40°) and both arms form a wider angle than the *ayin* of scribe B. The tip of the right arm on frg. 3a is more bent to the left. The diagonal stroke does not get finer towards the left end as it does on frg. 1. Despite the fact that all other letters are much taller, *ayin* of frg. 3a is smaller.



While the difference in each letter form on its own may seem very significant, the cumulative weight of the differences makes it quite probable that a third scribe C was responsible for frgs. 3a and 4d, which may or may not have belonged to a different scroll. If the palaeographical observations prove true, fragments 3a and 4d should receive a new independent abbreviation, e.g. *Mas 1q*. (15)

(14) The final *mem* has a maximal height of 5 mm on frg. 3a, 4 mm on frg. 1 i and 3.5 mm on frg. 1 ii, or 92 pixels vs. 64 and 78 pixels.

(15) A fresh codicological analysis should follow this palaeographical focus. The newly developed micro X-ray fluorescence spectrometer-technique might not necessarily be of much help to confirm or disconfirm these distinctions as it is unsure

As paleographical date, the transition period from Hasmonean to Herodian style as suggested by Talmon seem not out of the way. (16) With regard to his use of an almost perfect square final *mem*, scribe B could be a bit younger. However, the complete absence of *keraia* and the old-fashion base stroke of *bet* makes a date later than the final decades of the first century BCE unlikely.

Two scribes on a scroll or fragment is a rare phenomenon. *Mas Im* frg. 1 joins a relatively exclusive list of, so far, less than ten Dead Sea Scrolls. (17) That half of this group consists of “full scale” scrolls as opposed to “fragmentary” scrolls should serve as a warning with regard to the reliance on palaeography in the reconstruction of scrolls. After all, “fragmentary” scrolls are much more numerous than “large” scrolls. Consequently, less than one percent of all Qumran “fragmentary” scrolls appear on this list, (18) while about 20% of the “large” scrolls figure on it. (19) If we take 20% as the general ratio of scrolls written by more than one hand, a very high number of additional fragmentary scrolls (ca. 200!) might have been written by more than one scribe but we simply do not have a fragment displaying both hands together.

Most of the scrolls written by two or more scribes contain only one work. (20) Currently, relatively few scrolls containing more than one distinct work are known. (21) Still, sometimes a change in hands

whether the fragments stem from the same sheet of leather. On the application of this method to verify the attribution of Qumran fragments, see O. Hahn, T. Wolff, B. Kanngießer, W. Malzer and I. Mantouvalou, “Non-Destructive Investigation of the Scroll Material: ‘4QComposition Concerning Divine Providence?’ (4Q413)” *DSD* 14 (2007) 359-360.

(16) The early Herodian period seems less probable as *yod* and *waw* are usually well distinguished, there are no *keraia* at all and the base stroke of *bet* is still old-fashioned.

(17) The index of E. Tov, *Scribal Practices and Approaches Reflected in the Texts Found in the Judean Desert*, (STDJ 54; Brill, 2004), lists *1QH^a*, *1QpHab*, *4Q176*, *4Q216*, *4Q393*, *4Q448*, *8HevXIIgr*, *11QT^a*. (I exclude *1QS* and *1QIsa^a* as hand B of the former is a mere corrector and as I am not convinced that *1QIsa^a* has been written by two scribes).

(18) *4Q176*, *4Q216*, *4Q393*, *4Q448*, i.e. 4 of more than 900 scrolls.

(19) *1QH^a*, *1QpHab*, *8HevXIIgr*, *11QT^a*, i.e. 4 of 19 scrolls if we count *1QIsa^a*, *1QIsa^b* (*1Q8*), *1QpHab*, *1QapGen* (*1Q20*), *1QS*, *1QM* (*1Q33*), *1QH^a*, *3Q15*, *11Q1* (*paleoLev^a*), *11Q4* (*Ezek*), *11Q5* (*Ps^a*), *11Q10* (*tgJob*), *11Q11* (*apocrPs*), *11Q17* (*ShirShabb*), *11Q18* (*NJ*), *11Q19* (*T^a*), *Mur 88* (*XII*), *8HevI* (*XII gr*), *Mas 1h* (*Sir*).

(20) Not considering opistographs, on which cf. Tov, *Scribal Practices*, 68-73 and 295f.

(21) Tov, *Scribal Practices*, 39 mentions *4Q305*, *4Q319* and *4Q259*, *4Q392* and *4Q393*, *4Q419*, *Mur 24*. In addition, the *verso* of *4Q509+505* contains *4QM^f* and *4Q506*. With regard to closely related compositions, Tov mentions some scrolls containing more than one book of the Tora, the Minor Prophets or the “megillot” (165). The various writings collected in *1QS* belong here, too.

may indicate a switch to a different composition. (22) Therefore, the two columns of *Mas Im* frg 1 do not necessarily belong to the same composition.

If our paleographical observations are correct, what is the reason that Talmon did not publish these fragments as different texts? Maybe what mislead Talmon was the otherwise seemingly easy task of separating manuscripts at Masada. (23) Scrolls were found in eight different locations, a helpful fact for distinguishing between scrolls. At least five loci contained fragments stemming from one book, only. Three of these books are (para-)biblical and therefore easily identified. (24) The two other works were previously unknown but consist of just one fragment each. (25) Only two loci contained fragments of more than one scroll. The two biblical writings found in the synagogue are again quickly identified. (26) And also the seven books from casemate 1039 are also rather easily distinguishable: Three are biblical writings (*Mas I, Ia, Ie*), one is a copy of the *Songs of the Sabbath Sacrifice* already known from Qumran (*Mas Ik*), one is a papyrus (*Mas Io*), and the remaining two manuscripts have clearly distinct scripts (*Mas Il, Mas Ip*). In addition, all these scrolls survive only in one or two fragments. (27) The only locus, where more than one fragment was found and all were (apparently wrongly) ascribed to a single yet unknown composition, is casemate 1045 with *Mas Im*. As a ramification, we should remind the reader interested in the overall analysis of the Qumran corpus, that previously known compositions (Bible, *I Enoch, Jubilees*) are almost certainly overrepresented among the named Qumran fragments as even very tiny bits and pieces can be identified, which is not so with previously unknown texts.

Finally, some adjustments or alternative readings those in the editio princeps seem in order:

Frg. 1 ii line 1: The last two letters of II line 5 might be דש

Frg. 2a: the first line could be הנהם. The *he* in the third line could well be followed by a final *mem* instead of the suggested *nun*. An *ayin* is the last readable letter in the line, but it is the first letter of a word.

(22) This may be the case in 4Q393 and 4Q448, which is 25% of all scrolls written in more than one hand.

(23) The task of classifying the unknown writings, however, was rather difficult.

(24) Locus 92: *Mas Ib* (Lev 8-11): 40 frgs. Locus 1103: *Mas If* (Ps 150): 2 frgs. Locus 1109: *Mas Ih* (Sir): 26 frgs.

(25) *Mas In*, Unidentified Qumran-like text; *Mas Ij* – *Jubilees* or *Ps-Jubilees*.

(26) Locus 1043: *Mas Ic* (Deut 33-34): 4 frgs and *Mas Id* (Ezek 35-38): more than 50 frgs.

(27) *Mas Ie, Mas Il, Mas Ip* have two fragments.

Fig. 2b: There is also an *aleph* above the remains of the *lamed* and the final *mem*.

Fig. 2e: In line one, the long *vacat* behind the letter traces is noteworthy. The second line may read וְהָיָה כִּי־יִרְכֹּם. Other options for the letter before *resh*, such as *bet* or *ayin* should have left different traces on the parchment. The letter before *kaf* could be *resh* or *dalet*, but there are no real words with the latter. The letter behind *kaf* could be *waw* or *yod*, but the former does not give us good possible readings. The only letters to match the traces of the final letter are *dalet*, *zayin*, *mem* or final *mem*, and *resh*.

Fig. 4c: The first line looks rather as שְׁמַעוּ וְהָיָה. The last letter could be *lamed* or *qof*.

Mas 1q frg 1 (ex *Mas 1m* frg 3a): The first letter of the first line is completely unclear. The second line has to read וְהָיָה כִּי־יִרְכֹּם, *ayin* being the only possible letter that fits the remains of the stroke between *resh* and final *mem* to the right of the *mem*. The letter before *resh* could be pretty much everything, a *yod* not being the worst suggestion. The third line is difficult, especially the two letters after *mem*. The reading מִצְרַיִם suggested by Talmon seems quite impossible. One solution might be מִתְחַוֶּה, but the left downstroke of *taw* should have left a less diagonal mark (is some ink missing here?).

Should we read מִצְרַיִם or מִצְרַיִם or even מִצְרַיִם or יָם?

Mas 1q frg. 2 (ex *Mas 1m* frg 4d): The second line should read וְהָיָה כִּי־יִרְכֹּם. The letter before *tet* can only be a *bet*, a *kaf*, or a *pe*. There are scant remains of letters in the line below, וְהָיָה with a very uncertain *bet* preceding.

Fig. 5: The second word in the first line could also be *מָוֶת* or even *מָמוֹת*.

Fig. 6: The first line is much less certain than indicated.

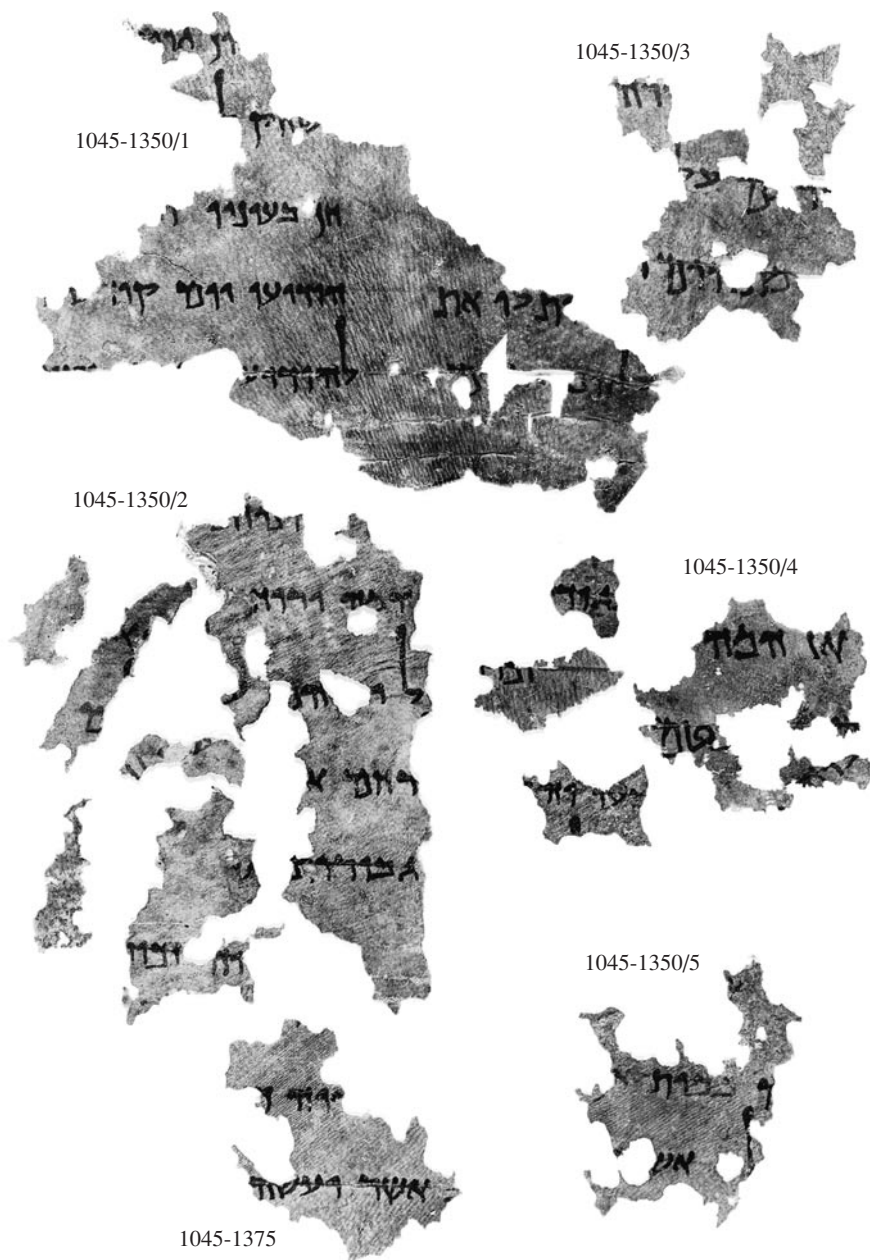
With regard to classification of genre and content, Talmon's tentative suggestion to associate these fragments with a (Sectarian) *Genesis Apocryphon* does not appear superior to Yadin's proposal to see in them an *Esther Apocryphon*. *חַן בַּעֲיִנִּי* and *תְּלֹה* from frg. 1 appear some verses apart in the story of Joseph and in Esther. However, as Talmon himself observes, the order *תְּלֹה אֵת* (hang [the baker]) before *חַן בַּעֲיִנִּי* makes an association with the Joseph story rather difficult. We cannot even be sure that frg. 1 contains one composition incorporating the two phrases *תְּלֹה אֵת* and *חַן בַּעֲיִנִּי*, as the two phrases appear in two different columns written by two different hands. Talmon's second argument for an association with Genesis, his restitution of *מִצְרַיִם* in line 2 of frg. 3a seems paleographically rather unlikely. Moreover, as this fragment does not seem to have been written by the same hand as frg. 1 and may not belong to the same scroll we cannot use it to analyze frg. 1 even if we should adopt a strange *מִצְרַיִם* for line 3. Finally, his explanations for the use of *יָדַע* *hif'il*, for *שָׁחַק* (frg. 1) and for *אֲשֶׁר יַעֲשֶׂה* (frg. 6) are not very convinc-

ing. (28) Yadin's association of frg. 1 with the book of *Esther* is as tentative and as good as Talmon's proposal.

In sum, the positive result of this palaeographical examination is that we should regard frgs. 3a and 4d of *Mas 1m* probably as independent scroll *Mas 1q*. We have, therefore, at least one more scroll from Masada, bringing the total number of scrolls from that site from 15 up to at least 16. We also have another case for a scroll written by two scribes (frg. 1). On the negative side, regrettably little new can be said about the possible identification of the two or more scrolls.

Daniel STÖKL BEN EZRA

(28) The object of *יום קודשו* is *הודיעו*. This expression is frequent in Sectarian texts, not something like "all this" as in *Gen* 41:39f. *שק* stands here with the preposition *ל* not *ב* as in *Gen* 39:14.17.



LE TESTAMENT DE LÉVI ARAMÉEN, CAMBRIDGE A-B ET F

Corrigenda et addenda

DANS ma note sur les colonnes du *Testament de Lévi* araméen de la Geniza du Vieux Caire, (1) j'avais fait des propositions de lecture des passages lacuneux. Concernant les colonnes a et b de la Bodleian Library et les colonnes c-e de Cambridge les restaurations me paraissent assurées autant qu'il est possible dans ce genre d'exercice, d'autant que des fragments de 4Q213 1 i-ii et 4Q214a 2 ii permettent de préciser certaines lectures en Cambridge e-f en particulier (2) Concernant les colonnes a et b de Cambridge, les restaurations proposées des lignes 15 à 19 en particulier sont plus difficiles puisqu'il manque environ la moitié des lignes, sans parallèle exact, et que le contenu de ces lignes reste en partie isolé. Le passage des *Testaments des XII Patriarches*, Lévi 6,2-5 en grec, est très résumé. Depuis cette note, plusieurs m'ont fait remarquer que mon interprétation de Camb. a 15 s'oppose à toutes les attestations de l'incident relatif au viol de Dînah à Sichem. Je propose ici un certain nombre de corrections qui touchent à ce point et à quelques autres pour avancer la compréhension de ces deux passages fort lacuneux ainsi que le début de la colonne f.

(1) E. Puech, "Le Testament de Lévi en araméen de la Geniza du Caire," *RevQ* 80 (2002): 511-556.

(2) Voir *ibidem*, Quelques précisions sont à apporter: p. 527, Bodl. b ligne 14, *hywdhr* est à lire *hy<<z>>dhr*, p. 538, Camb. d ligne 2, lire peut-être *lmn]'lynh* aussi possible que *lm]'lynh*, et p. 539, lire "*lmn]'lynh*, comparer K. Beyer, *Die aramäischen Texte vom Toten Meer* (Göttingen, 1984), 203-204 *lm]'lynh*". Mais pour l'espace est exclue la lecture trop courte *h]'lynh* de J.C. Greenfield, M.E. Stone and E. Eshel, *The Aramaic Levi Document. Edition, Translation, Commentary* (Leiden, 2004), 98 et 196, à la suite de Charles. Et pour la traduction de Bodl. a lignes 11-12, voir E. Puech, "Notes sur le Testament de Lévi de la grotte 1 (1Q21)," *RevQ* 82 (2003): 297-310, p. 298, note 2.

Cambridge, colonne a

- Ligne 15: Dans ma note j'étais en effet conscient de la difficulté qu'on a soulevée à propos de *Gn* 34,13 et de ses parallèles en *Jubilés* 30,3 et *Test. XII Patr.*, *Lévi* 6,3, (3) et j'avais essayé de m'en expliquer dans le commentaire. (4) Comme la lecture *ḏṯm't* s'impose, en l'absence d'un emploi passif connu du verbe j'optais pour le *pa'el* (5).

(3) Voir en dernier lieu, U. Schattner-Rieser, "J.T. Milik's Monograph on the Testament of Levi and the Reconstructed Aramaic Text of the Prayer of Levi and the Vision of Levi's Ascent to Heaven from Qumran Caves 4 and 1," *The Qumran Chronicle* 15 (2007): 139-154, p. 154. Mais cette note n'est pas sans erreurs, sans qu'on puisse savoir si elles viennent de l'original de Milik ou de la copie qui en est donnée, par exemple p. 148 ligne 12 (restauration de 4Q213a f2 1), lecture de la ligne 13, toutes reprises en U. Schattner-Rieser, "Remarques préliminaires sur le Testament de Lévi, monographie inachevée de J.T. Milik et quelques restitutions du Document araméen supposé original (4Q213A frag. 1-2)," *The Polish Journal of Biblical Research* 6/2 (2007): 113-121, p. 116 et 117. En dehors des fautes typographiques, cette dernière note corrige indûment le texte de Milik, p. 116 «4Q213a 2 4», voir p. 121 note 13, d'après l'araméen des targums, mais l'auteur ne doit pas être suivie pour cette correction d'après l'araméen tardif. Elle corrige en *'dkr* au lieu de *dkwr* lu par Milik, or dans la note précédente en *QCh, cit.*, p. 148, verset 17, on lit *dkr*, etc. Il est ainsi impossible de connaître ce que Milik a vraiment lu et restauré. Or à Qumrân, la forme *dkwr* est bien attestée, voir 4Q569 1-2 8. Dans cette note, p. 141, on donne l'opinion de Milik «TL is directly dependent or close to the original Aramaic document and that there are only few Christian interpolations», puis «Like Pierre Grelot Milik believes that the "original shape" depends on TL»! Il y a là une contradiction même si Milik a proposé une séquence comparable des événements dans les deux compositions, et je doute qu'il ait employé l'expression «Document araméen» au lieu de «Testament araméen». Par ailleurs dans "Notes sur le Testament araméen de Lévi. (Fragment de la Bodleian Library, colonne a)," *RB* 63 (1956): 391-406, p. 405 (je donne la référence complète car elle est, elle aussi, erronée à la note 10 de la p. 141), P. Grelot a écrit: «Le Testament araméen présente en de nombreux endroits un caractère secondaire par rapport à une source dont les Jubilés ont parfois mieux conservé la teneur, tout en la complétant sur certains points (détails sur la loi des Dîmes)». Ce qui est tout autre chose! Si l'auteur ne connaît pas les raisons pour lesquelles Milik n'a jamais publié cette étude ainsi que d'autres textes, d'autres les connaissent bien, c'est à lui qu'avait été confiée la publication officielle de ces manuscrits. Aussi dans ma note précédente étudiant les folios de la Geniza, je me suis bien gardé de toucher aux fragments du lot Milik! Cela laisse présager d'une publication quelque peu tronquée de l'œuvre que Milik m'avait montrée au début des années 1980 et plusieurs fois ensuite avec de nombreuses notes marginales écrites à la main. Enfin, on est surpris et amusé par la déclaration de Kapera in J.T. Milik, "Traduction continue du Testament de Lévi", avec introduction par Z.J. Kapera, *The Qumran Chronicle* 15 (2007) 5-24, p. 5: «I found it by chance in October 2006 among the papers left by Milik in his study. It was kept in a file titled TESTAMENT OF LEVI».

(4) Voir Puech, *cit. RevQ* 80, 516.

(5) La lecture *ṯm't* a été retenue, faute de mieux, par H. Drawnel, *An Aramaic Wisdom Text from Qumran. A New Interpretation of the Levi Document* (JSJSup 86; Leiden 2004), 229 et 356-57, et elle est acceptée maintenant par Greenfield-Stone-Eshel, *op. cit.*, p. 56-57: «you/she defiled the sojns of (?)», et p. 110-11, où il est expliqué que le verbe peut être la première ou la deuxième personne, même si la traduction propose la troisième personne! Mais ils n'acceptent pas la lecture du relatif *d-* qui pourtant ne pose pas de difficulté paléographique insurmontable (malgré leur

La lecture *'tm't* «fut souillée» de Milik (6) comprise comme un *'etpe'el* (ou *hof'al*) est matériellement exclue dans l'état présent du manuscrit. Il n'y a aucune trace du jambage gauche du *'alef*, et le bas de la haste plutôt verticale ne convient pas à l'axe d'un *'alef* mais bien mieux à un *dalet*. En araméen *tm'* est connu au *pa'el* (1Q20 XX 15) et au *'etpa'al* (4Q531 1 1 et 6 4, 4Q557 1 5), (7) au *pe'il* et au *'etpe'el* (8) et en syriaque au *pa'el*, *'etpa'al* et au *'af'el*. (9) Pour le sens passif, faudrait-il envisager une forme *pu'al*, (connu en hébreu à côté du *nif'al*, mais conjugaison très rare en araméen), ou envisager une correction supra-linéaire pour un *'etpe'el/'etpa'al*: *d[<>]tm't*? Précédemment je n'avais pas osé envisager une correction dans une restauration, mais elle semble nécessaire ici pour le sens conservé dans les textes parallèles. Dans ce cas il faut comprendre la ligne 14 comme *e. g. w'nh lwy hšbt 'l dynh 'htn'* 15 *d[<>]tm't l-*, «et moi, Lévi, je réfléchissais/machinais au sujet de Dînah, notre sœur,] qui a [*été*] souillée par-».

À la cassure, la lecture *lbn[* paraît bien préférable à *lbr[* pour laquelle on aurait une épaule très anguleuse de *reš*. Aussi de préférence à *lbr[nkr* «par un ét[ranger» (10) (*Gn* 34,2-3,7,13, *Jubilés* 30,2), on préférerait lire *lbn[y nkr* «par des ét[rangers», voir *Jubilés* 30,3-4-5 qui use du pluriel (mais plus difficilement *lbn[nkr*, un hébraïsme). (11)

- Ligne 16: Le mot *dbrt* suivi de *dy kl 'nšl* que j'avais analysé comme la locution composée, (12) peut aussi être analysé comme verbe «je décidai que», probablement suivi de *ygzwr* «devait être circoncis», voir *Gn* 34,15 contrairement au *Test. XII Patr.*, *Lévi* 6,3 qui lit la négation, et où il est aussi question de Ruben, (et ici aux lignes 17-18): «Après cela, je conseillai à mon père et à mon frère Ruben de dire aux fils de Hamor de (ne pas?) (13) se faire circoncirer

remarque à mon encontre, p. 111), ce ne peut être le pied du *lamed*, et ils laissent envisager la présence de deux lettres!

(6) Milik, "Traduction continue", *cit.*, 5-24, p. 11, et U. Schattner-Rieser, *cit. QCh*, 154.

(7) Voir M. Sokoloff, *A Dictionary of Jewish Babylonian Aramaic of the Talmudic and Geonic Periods* (Ramat-Gan 2002), 506.

(8) Voir M. Jastrow, *A Dictionary of the Targumim, the Talmud Babli and Yerushalmi, and the Midrashic Literature* (New York, 1950) I, 539.

(9) Voir R. Payne Smith, *Thesaurus syriacus* (Oxonii, 1879) I., col. 1484-85.

(10) Milik, "Traduction continue", *cit.*, 11.

(11) Greenfield-Stone-Eshel, *op. cit.*, 56-57 et 110-11, acceptent en principe ma lecture *lbn[y (?)*, au lieu de *yšl* précédemment, et traduisent: «you/she defiled the sons of (?)».

(12) Greenfield-Stone-Eshel, *op. cit.*, 56-57 et 111, ont accepté cette lecture.

(13) La lecture de la négation est discutée, certains manuscrits ne la portant pas, comparer les éditions de R.H. Charles, *The Greek Versions of the Testaments of the Twelve Patriarchs Edited from Nine Mss* (Oxford, 1908), 39 avec la négation retenue, et de M. de Jonge, *The Testaments of the Twelve Patriarchs. A Critical Edition of*

à cause de l'abomination qu'ils avaient commise contre ma sœur.» La première personne du singulier ne peut viser que Lévi à restaurer à la ligne 14. Pour l'espace, on comprendrait donc *w'm r'wbn 'hy*] 16 *dbrt dy kl 'nš[ygzwr*. (14)

- Ligne 17: À la cassure on peut hésiter entre *waw* et *yod* ou même, comme je l'ai proposé, un pied de *lamed* qui entraîna la restauration proposée, n'osant suivre une proposition déjà suggérée «according to the law of birth[right». (15) En fin de compte, celle-ci paraît préférable et elle est aussi retenue par Milik «et je le persuadai qu'il agît selon la loi de sa primogéniture». (16) On lira donc *kdyn bkw[rwth* ou *bky[rwth*. (17) La phrase serait à comprendre *e. g. wb'yt mnh*] 17 *lm'bd kdyn bkw[rwth* «et je lui demandai d'agir conformément à [son] droit d'aîne[sse]».

- Ligne 18: La compréhension de cette phrase dépend de la restauration. Je faisais alors appel au *Test. XII Patr.*, Lévi 6,3 qui reste tout de même une bonne indication, en comprenant «je consultai] Jacob mon père et Ru[ben mon frère sur cette/l'affaire (?)».

(18) Mais il est préférable de suivre Milik (19) en lisant plus simplement «et j'allai avec]Jacob, mon père, et Ru[ben, mon frère, auprès des fils de Hamor» *w'zlt 'm*] 18 *y'qb 'by wr'[wbn 'hy lbny hmr]*, restauration nécessaire pour expliquer le suffixe pluriel *lhwn*, à la ligne suivante, à moins de lire le gentilice, «les Sichémmites». Comme en *Test. XII Patr.*, Lévi, le passage fait intervenir Jacob avec ses deux fils, contrairement à Gn 34,13 où seuls les fils de Jacob s'entretiennent avec Sichem et Hamor.

the Greek Text (Leiden, 1978), 31, texte sans la négation. T. Baarda, "The Shechem Episode in the Testament of Levi. A comparison with other traditions," *Sacred History and Sacred Texts in Early Judaism. A Symposium in Honour of A.S. van der Woude* (ed. by J.N. Bremmer & F. García Martínez, Kampen, 1992), 11-73, p. 36-40, retient la négation comme préférable.

(14) Milik, "Traduction continue", *cit.*, 11: «Et avec Ruben mon frère je délibérais et nous décidâmes que chaque homme devait être circoncis». Mais l'espace est insuffisant pour une telle restauration de la ligne 15 avec deux verbes et à deux personnes différentes. Greenfield-Stone-Eshel, *op. cit.*, 56-57 et 111, acceptent aussi maintenant la lecture *kl 'nš*.

(15) Voir la traduction de J.C. Greenfield - M.E. Stone, "The Aramaic and Greek Fragments of a Levi Document", in *The Testaments of the Twelve Patriarchs. A Commentary*, by H.W. Hollander and M. de Jonge, Leiden 1985, 457-69, p. 461. Mais cette lecture n'a pas été maintenue par Greenfield-Stone-Eshel, *op. cit.*, p. 56-57 et 111-12, qui retiennent ma précédente proposition, *bkll*.

(16) Milik, "Traduction continue", *cit.*, p. 11.

(17) Voir *Targum Néophyti Gn* 25,31 et 34, et Sokoloff, *op. cit.*, 220: *bkyrw'*, syriaque *bkyrywt'*.

(18) Partiellement avec Greenfield - Stone, *cit.*, 461, retenu par Greenfield-Stone-Eshel, *op. cit.*, 57 et 112.

(19) Milik, "Traduction continue", *cit.*, 11: «Et je me rendis avec Jacob mon père et Ruben mon frère auprès des fils de Hamor.» Ici comme souvent ailleurs, Greenfield-Stone-Eshel, *op. cit.*, 57, se refusent à comprendre le passage.

- Ligne 19: Cette ligne ne fait aucune difficulté de lecture, et je maintiens ma précédente proposition: *w'mrnn lhwn b[hwkmh w]bynh dy h[ww]*, voir le *Targum Onqelos Gn 34,13*: *bhwkmt' wmlly dy ...*, et le *Targum Néophyti*: *bšgy hkmthwn wmlly dy....* (20)

- Ligne 20: Cette ligne n'offre elle aussi aucune difficulté. Toutefois Milik a analysé *brtn* comme un nom construit pluriel avec suffixe, ce qui ne peut être le cas. (21) La forme *brtn* peut être le pluriel absolu «des filles» ou le singulier construit avec le suffixe de la première personne du pluriel «notre fille» (= Dînah), voir *Gn 34,8,17*, *Jubilés 30,12*, mais pas «nos filles» (= *bntn*[']), voir *Gn 34,16,21*. (22)

- Ligne 21: La traduction de *whbryn* «et des partenaires» reprend une remarque de Greenfield-Stone, (23) puisque *Genèse 34* souligne tout autant une relation d'affaires entre associés, sens que la traduction «compagnon» ne comporte pas.

- Ligne 22: On doit noter la forme longue de l'impératif *hetpe'el* à orthographe pleine *whthmywn*, «être vu, être semblable, paraître, apparaître». (24)

- Ligne 23: La construction *bmylt qwšt* est indéterminée «par une circoncision de justice», non «par la circoncision de justice». (25) Le début du 'alef à la ligne 24 peut bien être le 'faux' début de 'hyn, mais à reporter à la colonne suivante. (26)

(20) Voir Puech, *RevQ* 80, *cit.*, 514, ligne 19. Milik, "Traduction continue", *cit.*, 11, ne lit que «et nous leur déclarâmes... à ce sujet que s'ils...». Greenfield-Stone-Eshel, *op. cit.*, 56-57 et 112, n'acceptent pas ma lecture de la lacune, pourtant évidente vu l'espace et les parallèles, mais uniquement ma lecture *dy* au lieu de *dw*. [de J.C. Greenfield and M.E. Stone, "Remarks on the Aramaic Testament of Levi from the Geniza," *RB* 86 (1979): 214-30, p. 216.]

(21) Milik, "Traduction continue", *cit.*, 11: «que s'ils désirent d'épouser nos filles et que nous devenions, frères et compagnons.»

(22) S'ils retiennent bien la traduction «notre fille», on ne comprend pas la lecture de Greenfield-Stone-Eshel, *op. cit.*, 56-57 et 112, qui corrigent 'ynwn «ils/eux» en 'ntwn «vous» et en conséquence lisent *h[n]* au lieu de *h[ww]* qui est bien plus logique dans cette séquence araméenne. Dans cette phrase on n'a pas affaire à un discours direct, qui ne commence qu'à la ligne 21.

(23) Voir Greenfield - Stone, *cit.* 1979, 217, repris et commenté en Greenfield-Stone-Eshel, *op. cit.*, 57 et 113, mais ils ne retiennent pas la graphie araméenne tardive *byšrkwn* qui paraît bien s'imposer, voir Bodleian b 11, à moins d'un effacement peu probable à première vue.

(24) Voir M. Sokoloff, *A Dictionary of Jewish Palestinian Aramaic of the Byzantine Period* (Ramat-Gan-Jerusalem, 1990), 205-06. Milik, "Traduction continue", *cit.*, 11, traduit en paraphrasant: «et montrez-vous circoncis».

(25) Milik, "Traduction continue", *cit.*, 11. Dans ma note *RevQ* 80, *cit.*, 517, corriger *Rm* 4,11 (au lieu de 31). Greenfield-Stone-Eshel, *op. cit.*, 56-57 et 115, préférèrent lire *[qš]wʾ* : si le *waw* est possible au lieu du jambage gauche du *šin*, en revanche le *qof* non lu est visible par le bas de sa hampe et on attendrait alors des traces du bas du *šin*. En conséquence, on préférera encore ma proposition.

(26) Voir Greenfield-Stone-Eshel, *op. cit.*, 116, explication préférable à celle que j'avais tenté dans ma note en *RevQ* 80, *cit.*, 518 note 12 à la suite de Beit-Arié, mais elle confirme ma compréhension du début de la colonne suivante (*RevQ* 80, *cit.*, 516), voir déjà K. Beyer, *Die aramäischen Texte vom Toten Meer* (Göttingen, 1984), 195.

Texte

- 15 ד[<א>]טמאת לבנ'י נכר ועם ראובן אחי]
 16 דברת די כל אנש[יגזור ובעית מנה]
 17 למעבד כדין בכו[רותה ואזלת עם]
 18 יעקב אבי ורא[ובן אחי לבני חמר]
 19 ואמרנן להון ב[חוכמה ו]בינה די ה[ווי]
 20 צביין אינן בברתן ונהוי כולן א[חי]ן
 21 וחברין גזורו עורלת בישרכון
 22 והתחמיון כואתן ותהון חתימין
 23 כואתן במילת קושט ונהוי לכון
 א
 1 [אחין וחברין]

Traduction

- 14 *Et moi, Lévi, je réfléchissais (?) au sujet de Dînah, notre sœur,]*
 15 *qui a [<été>] profanée par des ét[rangers (?), et avec Ruben, mon frère,]*
 16 *je décidai que chaque homme[devait être circoncis, et je lui demandai]*
 17 *d'agir conformément à [son] droit d'aîné[sse; et j'allai avec]*
 18 *Jacob, mon père, et Ru[ben, mon frère, auprès des fils de Hamor,]*
 19 *et nous leur parlâmes avec [discernement et]habileté, puisqu'ils*
 20 *désiraient notre fille et que nous deviendrions tous des frères*
 21 *et des partenaires: «Circoncisez le prépuce de votre chair*
 22 *et montrez-vous comme nous, vous serez alors marqués d'un sceau*
 23 *comme nous par une circoncision de justice, et nous deviendrons pour*
vous

- 1 *[des frères et des partenaires...]*

Comme il semble, la lecture et la compréhension de ce bas de colonne sont maintenant tout à fait récupérables pour le sens, malgré les critiques qu'ont m'a adressées de vouloir trop restaurer. (27)

Après une longue lacune de deux colonnes et 14 lignes, on pourrait peut-être insérer le texte conservé par le fragment de 4Q213a 3, (28) dans une certaine relation avec l'épisode du viol de Dînah, dans la mesure où le bas de la colonne Cambridge b 15-23 traiterait d'un

(27) Voir en particulier Greenfield-Stone-Eshel, *op. cit.*, 117-18, où il m'est reproché de ne pas mettre des points d'interrogation ou de ne pas signaler les points douteux; or mon texte en comporte en araméen et dans la traduction, en italique, je reconnais que j'aurais dû en mettre davantage, mais les crochets montrent bien où sont les restaurations.

(28) Pour la lecture de ce passage, voir E. Puech, *Qumrân Grotte 4 XXVII. Textes araméens, Deuxième partie. 4Q550-4Q575a, 4Q580-4Q587 et Appendices* (DJD XXXVII, Oxford 2008), 511-17, où j'apporte les corrections nécessaires à la présentation fautive de l'édition *princeps* de Greenfield et Stone (DJD XXII). Milik, "Traduction continue", *cit.*, 12, situe le fragment 4Q213a 3 après Camb. b et *Test. XII Patr.*, Lévi 6,6-7,4.

autre sujet. (29) Mais seule une lecture minutieuse de tous les restes conservés peut orienter vers une solution vraisemblable.

Cambridge, colonne b

- Ligne 15: Cette ligne comporte deux difficultés épigraphiques. Le dernier mot a généralement été lu *bkl* 'dn «en tout temps/ à tout moment», lecture que j'avais acceptée faute de mieux. Mais le *dalet* ne va pas de soi, car cette lecture laisse de côté non seulement le fin jambage gauche de la lettre et sans doute aussi une partie du *apex* touchant celui du jambage gauche du 'aïn lequel est de lecture assurée. Au lieu de *dalet*, je préfère de beaucoup y voir maintenant le tracé d'un 'alef: restes de l'axe, du jambage gauche sans *apex* visible mais pas toujours bien marqué (voir Bodl. c 7,22, etc.) et probablement les restes de l'*apex* du jambage droit touchant celui du 'aïn. La lecture *bkl* 'n paraît s'imposer (pour la séquence *bk*, voir Camb. a 17, c 16, Bodl. a 1, etc., à l'exclusion d'une séquence *bb*, voir Bodl. b 1,23, etc.). Ainsi une lecture *bbl*'wn n'est-elle pas à retenir. (30) Pour cette orthographe du mot, voir le cas emphatique à la ligne 23.

À la première cassure, il y a une trace d'encre correspondant à un départ de tête de lettre, au mieux soit *nun* final ou *he* ou *waw*, bien préférables à *pe* final précédemment retenu à cause de la lecture que je pensais possible à la ligne 19, mais à abandonner. (31) Dans cette séquence avec «mon frère» ou «mes frères», on pourrait penser à Ruben, Siméon et Judah parmi les frères de Lévi les plus vraisemblables dans un tel contexte. Toutefois Siméon qui exécuta les vengeances contre Sichem et Hamor (*Gn* 34,25-26,30, *Test. XII Patr.*, *Lévi* 6,4 et *Jubilés* 30,4) ne conviendrait pas dans un contexte en rapport avec l'épisode de Dînah. (32) On pourrait aussi hésiter entre les deux autres frères, en supposant qu'ils sont nommés. Étant donné que Judah quitte précipitamment le troupeau (ligne 23) et qu'il intervient déjà à la ligne 19, on pourrait envisager d'avoir affaire à lui ici aussi. La séquence *bkl* 'n, apparemment à l'état construit (comparer ligne 23) à compléter très vraisemblablement par ['bwnn, ferait alors référence à une responsabilité «sur le troupeau de [notre père» (Jacob), charge de la garde ou de soin du troupeau qui pourrait aussi incomber

(29) Dans ma note précédente, j'avais cru pouvoir la rattacher à l'histoire de Joseph, mais on doit abandonner cette lecture, même si Joseph intervient dans les guerres de Jacob (*Jubilés* 34,3). De leur côté, Greenfield-Stone-Eshel, *op. cit.*, 117-19, estiment «unlikely that it would still be discussing the Dînah incident». Ils pencheraient pour l'épisode des guerres des fils de Jacob contre les Amorites.

(30) Voir Milik, «Traduction continue», *cit.*, 11: «...mes sept frères se rassemblèrent à Bal'ôn (sans doute pour Bal'ôn?)». Greenfield-Stone-Eshel, *op. cit.*, 58 et 119, ont retenu la lecture 'dn.

(31) Drawnel, *op. cit.*, 230-33 et 356-57, a essayé de rendre compte de ma proposition, mais je suis responsable, non l'auteur, de cette fausse piste.

(32) Proposition de Beyer, *op. cit.*, 195.

en priorité à l'aîné des frères, Ruben. En effet, ailleurs, Ruben agit en responsable de ses frères, voir *Gn* 37,21-22,29-30; 42,22,37, mais cette surveillance du troupeau incombe aussi aux autres frères, comme c'est le cas en *Gn* 34,5,7. Dans ce cas, on pourrait comprendre *e. g.*: «tandis qu'avait la charge Rube]n, mon frère, de tout le troupeau de». Toutefois, compte tenu du sens entrevu par la suite, la lecture 'hy au pluriel (33) paraît préférable ici, on lirait donc le *waw* d'un verbe au pluriel, *e. g.* «Moi, Lévi, tandis que]mes frères[avaient la garde] de tout le troupeau de» 'nh lwy kdy 'zdhr]w 'hy bkl 'n.

- Ligne 16: Des traces à la cassure, on peut hésiter entre *yod* et le pied du *lamed* que suit 'alef, soit entre le pluriel à l'état emphatique]y' et, sans doute, précédé peut-être d'une fine trace de la base de *kaf*, le pronom]kl'. Une lecture possible serait ['bwnn qtl̄y]kl' dy hww bškm. En effet, Jacob avait acheté un champ en face de Sichem où il avait planté sa tente (*Gn* 33,19), ce qui expliquait la présence de Dînah dans la région et sa rencontre avec Sichem. Par ailleurs, dans l'épisode des guerres de Jacob, *Jubilés* 34,1,5 et 7 situe les pâturages de Jacob dans la région de Sichem où ses fils font paître les troupeaux, alors que Jacob, Lévi, Juda, Joseph et Benjamin sont à la maison auprès du vieil Isaac. Dans cette proposition, l'espace ne permet pas de lire les substantifs *gbr]*y' ou 'nš]y' même sans le *lamed* accusatif, à moins de lire *qtl̄t* (sans *yod* contrairement à Camb. d 17 et même encore écrit dans un module quelque peu réduit). Les parallèles connus font mention des «hommes, des mâles, et de tous», *Gn* 34, 20,25,26, *Jubilés* 30,4: «Puis Siméon et Lévi entrèrent à Sichem par surprise, exécutèrent le verdict sur tous les mâles de Sichem, tuant tous les hommes qu'ils y trouvèrent sans en laisser un seul, ils les tuèrent tous parce qu'ils avaient violé leur sœur Dinah», *LAB* 8,7, Flavius Josèphe, *Ant.* I §340, Démétrius (34), Théodote (35), *Pirqè de Rabbi Éliézer* (36), *Test. XII Patr.*, Lévi, 6,4-5. Alors que les textes attribuent aux deux frères le meurtre des deux personnages principaux (*Gn* 34,25-26,...) ou de tous les hommes de la ville

(33) Lecture envisagée avec hésitation par Greenfield-Stone-Eshel, *op. cit.*, 59: «. my brother(s?) at all times», et déjà par R.A. Kugler, *From Patriarch to Priest. The Levi-Priestly Tradition from Aramaic Levi to Testament of Levi* (SBL Early Judaism and Its Literature 9; Atlanta, 1996),63: «my brother(s)», ainsi qu'à la ligne 17.

(34) Voir Eusèbe de Césarée, *Préparation évangélique* IX 21, 9: «Syméon... et Lévi... tuèrent Emmor, Sichem son fils et tous les mâles pour venger le viol de Dina».

(35) D'après Alexandre Polyhistor, voir Eusèbe de Césarée, *Préparation évangélique* IX 22, 10: «Donc Lévi et Syméon entrèrent en armes dans la ville et tuèrent d'abord les premiers qu'ils rencontrèrent; après quoi ils massacrèrent Emmor et Sichem».

(36) *PRE* 288: «Et chacun, Siméon et Lévi, prit son épée et tua tous les hommes de Sichem».

de Sichem (*Test. XII Patr.*, *Lévi*, 6,4-5,...), dans mon hypothèse de lecture de ces lignes, l'araméen met en avant Lévi qui prend l'initiative du meurtre de Sichem et de tous les hommes de la ville, comparer ensuite *Test. XII Patr.*, *Lévi* 5,3-4: «Alors l'ange me fit descendre sur la terre. Il me donna un bouclier et une épée et me dit: “Venge sur Sichem, Dinah, ta sœur, et moi je serai avec toi, car le Seigneur m'envoyé”. Je massacrai, en ce temps-là, les fils de Hamor, comme il est écrit sur les tables célestes», et 6,4: «Je tuai Sichem pour commencer, et Siméon tua Hamor». (37) Il est difficile de retenir la proposition de Milik: «car ils ignoraient tout des bataill[es] qui avaient eu lieu à Sichem». (38)

- Ligne 17: Cette ligne paraît a priori difficile à comprendre. On peut suivre en partie la proposition de Milik: «Mais arrivé là-bas Juda] mon frère se mit à raconter: Tel», en lisant plus simplement pour l'espace *e. g.* *'dyn 'zl / tb (?) yhw dh] 'hy w' hwy dn*: «Alors partit / s'en retourna (?) Judah,] mon frère, et il rapporta: “Ceci...”». (39) Cette proposition ne présuppose pas la restauration de *yhw dh] 'hy* à la ligne 15.

- Ligne 18: Au lieu de la lecture *šm] '[y]n*, je propose maintenant de lire les restes d'encre comme *'aïn* et *lamed*, voir la séquence *e. g.* Camb. c 13,14,16,22, d 22, etc., restes qu'on peut comprendre *dn [pš]' l*: «Ceci/c' (?) (est) [une offen]se (faite) à/contre[»]. On pourrait alors compléter soit *l[y'qb 'bwnn] bškm* «à[Jacob, notre père,] à/par Sichem», soit encore *l[dynh 'htn'] bškm* «à[Dînah, notre sœur,] à/par Sichem (a priori toponyme sans pouvoir exclure encore l'anthroponyme)», voir *Test. XII Patr.*, *Lévi* 6,8; 7,3, *Targum Néophyti* 34,31, mais en estimant que le passage se rapporte encore à l'affaire du viol de Dînah (voir ligne 20, et *Jubilés* 30,5). (40) *Gn* 34,5-6 rapporte que Jacob garda le silence du viol de Dînah sa fille, jusqu'au retour de ses frères de la campagne. On sait par *4Q213a* 3 qu'une telle abomination porte atteinte aussi bien à la fille profanée qu'à son père, à ses ancêtres et à toute sa maison. (41) Dans le cas de la lecture «Jacob» qui paraît préférable à la ligne 18, l'offense devrait alors viser la trahison de la parole donnée dans la vengeance et la violence des fils de Lévi contre Sichem et Hamor afin de ramener leur sœur

(37) Alors que *Judith* 9,2 met en avant Siméon, de même Théodote, Eusèbe de Césarée, *op. cit.*, IX 22 8-9.

(38) Milik, “Traduction continue”, *cit.*, 11, faisant peut-être référence à *Gn* 34,30-31.

(39) Greenfield-Stone-Eshel, *op. cit.*, 119, accepteraient aussi comme possible la lecture «Dan», sans doute en référence à sa présence en *Test. XII Patr.*, *Judah*, 7,2 et 6.

(40) Il est difficile de retenir la proposition de Milik, “Traduction continue”, *cit.*, 11: «Tel [était le combat contre Hamor et tel] contre Sichem».

(41) Pour la lecture de ce passage, voir E. Puech, *op. cit.*, DJD XXXVII, 511-17, où je réduis au fragment 3 les fragments 3-4 des éditeurs, auxquels on doit encore joindre un autre fragment non identifié parmi les fragments du ‘lot Starcky’.

Dinah après la circoncision des Sichémmites, cause de la réprimande de Lévi et Siméon par leur père, voir *Gn* 34,30, *Test. XII Patr.*, *Lévi* 6,6 et surtout 6,7, comme ils le reconnaissent eux-mêmes: «Car nous avons péché, puisque nous avons fait cela contre sa volonté. Il tomba malade ce jour-là» ἡμάρτομεν γάρ ὅτι παρὰ γνώμην αὐτοῦ τοῦτο πεποιήκαμεν· καί γε ἐμαλακίσθη ἐν τῇ ἡμέρᾳ ἐκείνῃ. (42) Comme le substantif *pš'* n'est pas encore attesté en araméen, mais uniquement le verbe, on doit sans doute lire ici le verbe «agir incorrectement, transgresser» (43) et comprendre *dn*, non comme le démonstratif neutre «ceci» mais au sens personnel «celui-ci», visant alors, dans la bouche de Judah, son frère Lévi; une lecture [*pš'* *n'*] n'est pas possible pour le pluriel. Judah n'ayant pas été mis dans le coup, on comprend mieux qu'il rapporte ces faits commis par son frère Lévi ou ses frères, Lévi et Siméon. Ainsi, alors que les deux propositions [*y'qb' bwnn*] / [*dynh' htn*] sont matériellement et contextuellement a priori possibles, l'une paraît toutefois plus vraisemblable que l'autre. En conséquence, le passage se réfère donc encore à l'épisode du viol de Dinah, même s'il vient après plus de deux colonnes de texte.

- Ligne 19: Au lieu de ma précédente proposition, difficile pour toutes les traces, je lis au mieux *mtqb'* avec le long tracé à gauche du 'aïn à la cassure au lieu de l'oblique du *samek*. Le verbe *qb'* signifie «fixer, établir, déterminer, tramer, combiner». Cette lecture me paraît rendre mieux compte des traces que *mt'bd[yn hww]* supposée par Milik: «et ce qu'[on avait] fait[aux fai]seurs de violence», (44) le *dalet* paraît exclu à la cassure, comparer les séquences en Camb. a 17 d'une part et, d'autre part, le mot serait trop long pour l'espace. La lacune devrait se comprendre: *wmh mtqb' [hwh b'b]dy hms'* «et ce qui[avait été] arrêté/déterminé/tramé contre (?) les ar]tisans de violence», voir l'expression en Camb. d 18 visant les Sichémmites; voir encore *1QApGn* V 18 et X1 14 et *4Q531* 19 2, et le parallèle syriaque *lklhwn 'bdy 'wl'*. Mais l'expression peut difficilement ici désigner les deux frères Lévi et Siméon, voir *Gn* 49,5. Le verbe *mtqb'* paraît avoir été connu de la tradition reçue par Flavius Josèphe, *Antiquités juives* I §339-341: «(339) Le roi repartit donc, espérant que Jacob consentirait au mariage. Et Jacob, ayant instruit ses fils du déshonneur de leur sœur ainsi que de la demande de Hamor, les consulta sur ce qu'il convenait de faire. La plupart restèrent muets, ne sachant que penser, mais Siméon et Lévi, frères de la jeune fille de même mère, tramèrent (συντίθενται) ensemble l'opération suivante. (340) Pendant une fête,

(42) M. de Jonge, *The Testaments of the Twelve Patriarchs*, op. cit., 31. Cela semblerait appuyer la lecture sans la négation en 6,3. Cette offense contre la volonté du père est clairement soulignée par Flavius Josèphe, *Antiquités juives* I §340.

(43) Voir Sokoloff, *opera cit.*, 453 et 944 respectivement.

(44) Milik, «Traduction continue», cit., 11.

tandis que les Sichémites se livraient aux plaisirs et aux festins, (45) ils surprirent nuitamment les premiers gardes les tuant dans leur sommeil et, pénétrant dans la ville, ils tuèrent tous les mâles ainsi que leur roi et son fils, mais ils épargnèrent les femmes. Ayant fait cela sans l'assentiment du père, ils ramenèrent leur sœur. (341) Jacob fut bouleversé par l'énormité de ces agissements et fut très irrité contre ses fils,...». Ces parallèles renforcent la lecture paléographique de la ligne 19 et la restauration proposée pour l'espace.

- Ligne 20: Judah paraît faire un rapport à son père et à ses autres frères (*'ynwn*), tandis que la mention de Lévi et Siméon côte à côte paraît renvoyer à *Gn* 34,25, voir 34,30, (46) voir aussi *Jubilés* 30,6, voir aussi *IQ21* 48. (47)

- Ligne 21: Au lieu de *lhhdh* proposé précédemment, lire plus simplement *lhw[d]yh* avec de fines traces de la tête du *waw*, et une large tête du *yod* (comparer Camb. c 22, b 11, Bodl. a 15, b 2, c 13), sans départ de tête pour *dalet* parfois proposé. (48) Cette lecture évite la difficulté signalée du *he* au lieu du *'alef* attendu pour *l'hhdh* d'une part et, d'autre part, elle convient mieux au contexte, et pour la forme *lhwdyh*, voir *Tob* 14,2 (*4Q196* 18 15, *4Q198* 1 1). Judah rapporte que Lévi et Siméon sont allés remercier Ruben, leur frère aîné, pour l'autorité dont il a dû faire preuve en exerçant son droit d'aînesse (voir Camb. a 17) et pour le conseil-stratagème de la circoncision afin de faire réussir le plan de Lévi de venger Dînah, Camb. a 15-16. La proposition de Milik paraît bien moins satisfaisante: «que moi-même et Siméon mon frère nous étions partis exciter Ruben». (49)

- Lignes 22-23: La lecture *lmdnh* est assurée. (50) Au lieu de *'šr* «Asher» de ma précédente note, on devrait lire *'šl* «Eshel», avec de légères traces de la hampe du *lamed* dont je n'avais pas tenu compte précédemment, l'ayant considérée comme grattée et effacée, or ce pourrait bien ne pas être le cas. Avec *'šl*, on doit avoir affaire à un toponyme ou lieu-dit, «Le Tamaris/ Eshel», voir d'autres toponymes-homonymes en (*Gn* 21,33) *I S* 22,6 et 31,13, rendus dans le *Targum d'Onqelos* *'šl'* (transcription de l'hébreu en araméen au lieu de *'tl'*

(45) Flavius Josèphe semble ignorer la ruse de la circoncision du récit biblique qui est remplacée par une fête.

(46) Milik, "Traduction continue", *cit.*, 11, traduit: «"Tel était le combat contre Hamor et tel contre Sichem" et ce qu'on avait fait aux faiseurs de violence».

(47) Voir *RevQ* 82, *cit.*, 307.

(48) Voir Greenfield-Stone, *RB* 1979 *cit.*, 217: *lh* []*dd*, et Greenfield-Stone-Eshel, *op. cit.*, 58 et 120: *lh* []*rd*, proposant les verbes *mrd*, *trd* ou *srd*, ou même *'rk*, mais aucun de ceux-ci ne paraissant convaincant, ils n'excluraient pas un toponyme. Les restes des tracés n'appuient pas ces lectures. Toutefois, malgré ce qui est écrit p. 118, le *'af'el* de *'hd* existe, voir *Targ. Ps* 73,23.

(49) Milik, "Traduction continue", *cit.*, 11, mais on ignore pour le moment le verbe araméen lu dans ce cas.

(50) Avec Milik, "Traduction continue", *cit.*, 11, malgré Greenfield-Stone-Eshel, *op. cit.*, 120, toutefois les auteurs retiennent la lecture.

attendu pour «le tamaris»), ce qui évite de situer Ruben et son troupeau beaucoup trop au nord, à l'est d'Asher. (51) Cette lecture se comprend bien dans le contexte géographique des *Test. XII Patr.*, *Lévi*, 2,5 et 5,1 où Gebal = le mont Ebal, Abila = Abel-Maïn (= 'Ein Beit el-Ma'), et le mont Aspis = Har Garizin (voir *grzn* = arme) «au sud d'Abila». (52) En effet, les restes du jambage presque vertical de la lettre favorise nettement la lecture d'un 'alef et non d'un mem médian pour *mšl* - «Meshel» ainsi que le propose dernièrement Milik: «Ruben notre frère résidant à Ruben (*sic*) situé à l'est de Meshel». (53) La séquence *wšwr yhw dh qdm'*, littéralement «et Judah bondit en avant», se rend mieux en français par «et Judah partit précipitamment...». Judah partit précipitamment, abandonnant le troupeau, pour aller retrouver son père Jacob et l'informer, montrant à première vue son désaccord avec ses autres frères, Lévi, Siméon et Ruben. Il fera mieux que ses frères à son tour, voir *Test. XII Patr.*, *Judah*, 4,1-3.

Texte

15 אנה לוי כדי אודהר[ו אחי בכל עאן
16 [אבונן קטלית] כלא די הוו בשכם
17 אדין אול[תב יהודה] אחי ואחוי דן
18 [פש]ע ל[יעקב אבונן] בשכם ומה
19 מתקבע[הוה בעב]די חמסא ואחוי
20 אינן יהודה די אנה ושמעון
21 אחי אולנא להו[ד]יה לראובן
22 אחונן די למדנח[אשל ושור
23 יהודה קדמא [ל]משבק עאנא \\\

Traduction

- 15 *Moi, Lévi, tandis que* [mes frères [avai]ent [la charge] de tout le troupeau de
16 [notre père, je tui]ous (?) ceux qui étaient à Sichem.
17 [Alors partit Judah] mon frère, et il rapporta: «Celui-ci
18 [a péch]é à l'égard de[*Jacob, notre père*] à Sichem, et ce qui
19 a été tramé [contre les ar]tisans de violence». Et Judah

(51) Lecture retenue par Greenfield-Stone-Eshel, *op. cit.*, 120, qui citent des toponymes au nom de 'sr non loin de Sichem.

(52) Baarda, *cit.*, 20-25, hésite sur l'identification de ces toponymes, et cherche dans la région de l'Hermon et Abel-Meholah, convaincu que le mot Aspis renvoie à un bouclier. Sur ces toponymes, on doit suivre J.T. Milik, "Le Testament de Lévi en araméen," *RB* 62 (1955): 398-406, p. 403-05, et J.T. Milik, "Écrits préesséniens de Qumrân: d'Hénoch à Amrami" *Qumrân: sa piété, sa théologie et son milieu* (ed. M. Delcor, *BETL* 46; Louvain, 1978), 91-106, p. 96-7, même si on ne situe pas la composition en Samarie.

(53) Milik, "Traduction continue", *cit.*, 11, avec probablement une erreur de transcription du texte de Milik.

- 20 leur rapporta que moi-même et Siméon,
 21 mon frère, nous étions allés remer[ci]er Ruben,
 22 notre frère, qui était à l'orient du Tamaris/Eshel, et Judah
 23 partit précipitamment, laissant le troupeau.

Une lecture précise des restes de lettres permet, me semble-t-il, une approche très vraisemblable de ce bas de colonne et une réponse à une question essentielle, celle de l'épisode visé dans ces lignes. Il concerne encore l'affaire de Dînah à Sichem, sujet d'un long *midrash* sur cet incident. (54) Il en résulte que 4Q213a 3 doit alors plus vraisemblablement se placer dans les colonnes qui suivent. (55)

Cambridge, colonne f 1-16 (voir 4Q213 1 i 12 ss)

- Lignes 1-2: On peut discuter ma précédente restauration de ces lignes d'après le *Test. XII Patr.*, Lévi 13,9 (σύνθρονος ἔσται βασιλέων), en complétant à l'aide de 4Q213 1 i 12: *lyqr wlrwb wlmkyn* [, e. g. [*mt'hd hw'* ou [*mtqr' hw'* «à la gloire et à la grandeur et à/auprès des rois [il a été associé / il a été appelé]», mais on peut difficilement accepter la restauration [*y't hw'* vacat. (56)

- Lignes 2-3: En conséquence, restaurer le début de la ligne 2, e. g. '*lpw spr w*)'*l tmhwlw hwkmt' lm'lp* à l'aide de 4Q213 1 i 13, en reprenant partiellement la formulation de Camb. e 23 *dm'lp spr*, «Apprenez l'écriture et [ne négligez pas d'apprendre la sagesse]». (57)

- Ligne 3: Une lecture *wl'rḥth l'* *tšb[qw* en parallèle à '*l tmhwlw* paraît à retenir. (58)

- Lignes 4-5: Au lieu de ma précédente restauration, on peut compléter e. g. *lb[nykwn 'lypw hwkm]h*, en faisant de *gbr dy* le début

(54) Contrairement à la proposition de Greenfield-Stone-Eshel, *op. cit.*, 117-19.. Kugler, *From Patriarch to Priest*, *op. cit.*, 52-3, 57, 63-68, situe Camb. a et b avec l'incident de Sichem avant la prière et la vision de Lévi. Mais il ajoute, p. 66: «Unfortunately, because the text breaks off so soon it cannot be known whether *Aramaic Levi* preserves Jacob's reproof and Levi's self-justification (cf. *T. Levi* 6:6-7:4).» Voir aussi p. 201-03.

(55) Voir Milik, "Traduction continue", *cit.*, 11-12, et de même indépendamment Drawnel, *op. cit.*, 35, même si l'auteur suivait, faute de mieux, ma première lecture de Camb. b, voir p. 54 et 357-358.

(56) Greenfield-Stone-Eshel, *op. cit.*, 103-04 et 210. Le verbe *y't* gouverne l'accusatif (voir Sokoloff, *op. cit.*, 243a), or *lmkyn* est à l'état absolu qui ne peut être à l'accusatif. En outre, *wlmkyn* semble construit en parallèle aux deux substantifs précédents. Cette restauration laisse un *vacat* d'un cm dans une colonne de 12 cm en 4Q213 1 i 13, non marqué en Camb. f 1. Milik, "Traduction continue", *cit.*, 19, comprend: «comment son instruction en écriture et en discipline le fit se révéler sage, estimé et honoré, de sorte qu'il devint le second auprès du Seigneur des deux royaumes ayant été mis assis sur le trône de lieutenant.»

(57) Voir Greenfield-Stone-Eshel, *op. cit.*, 104-05 et 211.

(58) Greenfield-Stone-Eshel, *op. cit.*, 104 et 211, accepteraient ma proposition et restaurent *l'* *tšbq[wn*.

de la phrase suivante. (59) Le scribe de 4Q213 1 i 14 a d'abord copié]m'lp «celui qui apprend», corrigé en]m{' }<y>l'/d (?) (la tête du *pe* final a été corrigée en 'alef ou *dalet*, difficilement *kaf* final). Le mot]myl' peut être le substantif absolu «parole, matière, discours» qu'on pourrait peut-être retrouver en Camb. f 4 en lisant le cas emphatique mylt]h au lieu de hwkml]h.

- Ligne 7: Le copiste a certainement écrit]'ll lh (avec des traces de 'aïn et de *lamed*), non y'zl lh. (60) Le verbe 'll se construit aussi avec le *lamed* du lieu, alors que 4Q213 1 i 15 lit un synonyme, yhk lh précédé de šm']' lkl mt wmdynh avec des restes du 'alef, non de *he* des éditeurs. Puis lire certainement 'h 'w hbr comme je l'ai proposé, non 'a' w^abr, un substantif à l'état emphatique et un autre coordonné à l'absolu. (61)

- Ligne 8: Une restauration w[l' m]tnkr est à retenir pour les traces au lieu de [l' kw']t nkr beaucoup trop long pour l'espace disponible, (62) littéralement «et il [n']y est [pas] distinguable», phrase qui annonce les précisions des deux suivantes.

- Ligne 9: La lecture lkyl'[y]mn dy, avec de légères traces de 'alef (*apex* et base de l'axe), paraît plus probable, lecture qu'on retrouve en 4Q542 1 i 5-6: lkyl'yn en parallèle à lnkr'yn (63).

- Ligne 16: A la fin de la ligne, il semble possible de lire une addition marginale supra-linéaire [yd]'yh, possiblement surmontée de [l]kl à gauche d'un arrachement partiel de la surface, voir 4Q213 1 i 20 lkl] yd'yh. (64) L'expression est parallèle à lkl qnyh' de la ligne suivante.

(59) Greenfield-Stone-Eshel, *op. cit.*, 104, ne lisent pas le *he* à la cassure. Milik, "Traduction continue", *cit.*, 19, comprend: «Ne négligez pas d'apprendre la sagesse ni laissez vos fils mépriser l'intelligence. L'homme qui apprend la sagesse tous les jours voit son honneur grandir progressivement chaque semaine».

(60) Comme l'estiment Greenfield-Stone-Eshel, *op. cit.*, 104, 106 et 211. Il ne peut y avoir des restes de ces trois lettres en cet endroit.

(61) Voir Greenfield-Stone-Eshel, *op. cit.*, 104 et 211. Milik, "Traduction continue", *cit.*, 19, lit: «Quels que soit (*sic*) le pays ou la ville où il s'en ira, il y sera acclamé 'Mon petit frère!' et 'Mon copain!', il y sera reconnu et appelé 'Papa!'».

(62) Greenfield-Stone-Eshel, *op. cit.*, 104.

(63) Comme je l'ai expliqué en DJD XXXI, 268 et 273-74. Greenfield-Stone-Eshel, *op. cit.*, 104, 106 et 211, lisent lkyly]. Milik, "Traduction continue", *cit.*, 19, lit: «On ne l'y assimilera pas à un étranger ni à un métèque.»

(64) Comme je l'ai compris en RevQ 80, *cit.*, 555 et note, Greenfield-Stone-Eshel, *op. cit.*, 104, lisent]yd'yh, reprenant l'édition officielle de DJD XXII, 6 et 9, mais l'espace à la cassure s'oppose à la restauration du *lamed* préposition. Milik, "Traduction continue", *cit.*, 19-20, lit: «C'est à lui que tout le monde rendra l'honneur, car tout le monde désirera apprendre de sa sagesse. Ses amis seront nombreux et nombreux seront ceux qui le salueront. On le fera s'asseoir sur un siège d'honneur car beaucoup désireront le servir et dans un tribunal écouter ses sages paroles. C'est un grand trésor d'honneur qu'est la sagesse pour tous ceux qui apprennent à la connaître et un bon placement de gloire pour tous ceux qui l'acquièrent.»

Texte

- 1 ליקר ולרבו ולמלכין) מתאחד/מתקרא הוא
 2 אלפו ספר ו(אל תמחולו חוכמתא
 3 למאלף ולארחתה ל[א תשב] קו
 4 לב[ניכון אליפו חוכמ]ה/מילת[ה גבר ד[י
 5 אלף] חוכמה כל י[ומוהי א[ריכין]
 6 וסגה ל[ה שמ]עה לכל מא[ת]
 7 ומדינה [די] עלל לה אח או חבר
 8 הוי בה ו[לא מ]תנכר הוא בה
 9 ולא דמ[ה בה] לנכרי ולא דמה
 10 בה לכילא[י] מן די כולהון יהבין
 11 לה בה יקר[ב] די כולה צבין
 12 למאלף מן חוכמתה Vacat רחמוה[י]
 13 סגיאין ושאליו שלמיה רברבין
 14 ועל כורסי (ד) ייקר מהותבין לה
 15 בדיל למשמע מילי חוכמתה
 16 עותר רב די יקר היא חוכמתה <[ל] כל [יד] עיה>
 17 וסימא טאבא לכל קניהא הן

Traduction

- 22 Observez, mes enfants, Joseph, mon frère,
 23 qui enseignait écriture et instruction de sagesse;
 1 [à la gloire et à la grandeur et à/auprès des rois (*il a été associé/ il a été appelé*].
 2 Apprenez l'écriture et) ne négligez pas d'étudier
 3 (la sagesse, et ses voies n']abando[nnez] pas.
 4 À [vos] fi[ls enseignez]la [sagesse/][la[Parole]. L'homme qui
 5 apprend[la sagesse, tous]ses jours sont p[rolongés],
 6 et sa[réputa]tion croît. Dans chaque pays
 7 et contrée[où il en]tre, un frère ou un compagnon
 8 il a, et il [n']y est [pas] distinguable,
 9 et il n'[y] est pas semblable à un étranger et il n'y est pas
 10 semblable à un métèque, si bien que tous lui rendent
 11 honneur à cause d'elle, [étant donné] que tous veulent
 12 apprendre de sa sagesse. vacat Ses amis
 13 (sont) nombreux et, innombrables, ceux qui le saluent.
 14 Et sur un siège d'honneur on le fait asseoir
 15 afin d'entendre ses paroles de sagesse.
 16 C'est une grande somme d'honneur que la sagesse <[pour] tous ses
 [connai]sseurs>,
 17 et un excellent trésor pour tous ses acquéreurs. Si...

Testament de Lévi araméen

Il apparaît clairement que *Test. XII Patr.*, *Lévi* s'inspire de près de ces passages du *Testament de Lévi* araméen. Aussi je garde le sigle

Testament de Lévi araméen pour cette composition, au lieu de *Document de Lévi araméen* que d'autres ont proposé et tendent à imposer. (65) Malgré l'état fragmentaire du texte dont la structure générale se laisse assez bien retrouver à l'aide des copies qumraniennes et de la Geniza, des fragments grecs du Mont Athos et des lignes syriaques, il ne fait aucun doute qu'on a affaire à un *Testament*, même si le mot lui-même n'y apparaît pas, tout comme il n'apparaît pas dans les *Testaments des XII Patriarches*. Il en est de même en tête des *Visions d'Amram*: «Copie de l'écrit des paroles des visions d'Amram, fils de Qahat, fils de Lévi, tout ce qu'il déclara à ses fils et qu'il leur recommanda le jour de sa mort, en l'an cent trente-six, c'est l'année de sa mort...» (4Q543 1 et 4Q545 1, comparer *Test. XII Patr.*, Lévi 1,1-2: «Copie des paroles de Lévi, tout ce qu'il prescrivit à ses fils, en prévision de tout ce qu'ils auraient à faire et de tout ce qui devrait leur arriver jusqu'au jour du jugement. Il était encore en bonne santé, quand il les appela auprès de lui, car il lui avait été révélé qu'il était sur le point de mourir. Et quand ils furent réunis, il leur dit: ...»). (66) On doit avoir affaire à un des Testaments des *Trois Patriarches* mentionnés parmi les *Livres Apocryphes* dont il est question dans les *Constitutions Apostoliques* VI 16, 3a. (67) Les *Trois Patriarches* en question devraient être Lévi, Qahat et Amram, la trilogie sacerdotale, des ancêtres directs d'Aaron.

Datation

La datation de la composition du *Testament de Lévi araméen* est discutée. Les critères paléographiques des manuscrits qumraniens exigent une datation de la composition avant le milieu du II^e siècle. En effet, le *Testament de Lévi*, le premier de la trilogie sacerdotale, doit certainement être antérieur aux *Testaments de Qahat* et d'Amram dont les plus anciennes copies (4Q542, 4Q544 et 4Q547) datent de la deuxième moitié du II^e siècle, rejoignant la datation de la plus ancienne copie du *Testament de Lévi*. Or avec toutes ces copies, on n'a certainement pas affaire à des originaux araméens. La composition doit donc être plus ancienne. La prééminence de Lévi et de la classe sacerdotale est déjà bien soulignée dans la *Chronologie biblique* (4Q559), composition qui devrait dater du III^e siècle au plus tard, et elle semble bien connue du *Testament de Lévi* et des *Jubilés*. (68)

(65) Voir dernièrement encore Greenfield-Stone-Eshel, *op. cit.*, *passim*, et Schattner-Rieser, "Remarques préliminaires sur le Testament de Lévi...", *cit.*, 115, ou Schattner-Rieser, "J.T. Milik's Monograph...", *cit.*, 140-41, alors que dans ses publications Milik n'a jamais utilisé ce sigle!

(66) On comprend pourquoi Milik restaure ainsi le début de la composition, "Traduction continue", *cit.*, 7.

(67) *Les Constitutions Apostoliques*. Tome II, Livres III-VI. *Introduction, texte critique, traduction et notes*, par M. Metzger (Sources chrétiennes 329; Paris, 1986), 346-47.

(68) Voir Puech, DJD XXXVII, *op. cit.*, 263-89. On ne peut suivre J. Kugel,

Recensions qumraniennes?

Enfin, il est difficile d'accepter l'existence de plusieurs recensions de cette composition araméenne parmi les copies qumraniennes et celles de la Geniza. En effet, sans preuves claires, l'hypothèse récemment avancée de révisions qumraniennes en fonction des intérêts de la Communauté ne me paraît pas prouvée. (69) Avant de conclure en ce sens, pour apprécier le nombre et la nature des variantes textuelles, faut-il d'abord s'assurer de la lecture du texte recopié par les manuscrits qumraniens dont les éditions méritent une sérieuse révision, et apprécier des fautes possibles dues, en cours de transmission, au travail des scribes, ainsi que les simples variantes orthographiques. (70) Ainsi, on doute de la variante "essénisante" en 4Q213b 1 1. Au lieu de la lecture *r'ytk* de l'édition, on doit certainement lire *rbytk* «je t'ai magnifié». (71) De son côté, Bodleian a 7 (§6) lit le pluriel *rbynk* «nous t'avons magnifié». Comment rendre compte de cette variante? Parmi trois possibles explications, 1) correction volontaire du singulier au pluriel par le copiste de la Geniza, 2) erreur du copiste de la Geniza ayant introduit les sept anges auparavant, Kugler en retient une troisième, le scribe qumranien a fait le choix du singulier pour réduire l'interlocuteur à un seul ange ou même mieux à Dieu, en comparant la réécriture des 'lois mosaïques' à la première personne dans le *Rouleau du Temple*, ou des passages des *Pseudo-Jubilés* (4Q225 2 ii 8-10) ou encore de *IQpHab* VI 13-VII 5. (72) Il estime que cette variante prouve l'intervention réfléchie du copiste qumranien adaptant le texte à la perspective propre des compositions de la Communauté. Une telle conclusion me semble excessive avec cette unique variante sans autre preuve dans la composition. Une erreur de lecture ou/et de copie paraît tout aussi possible dans ce cas, la séquence *yod-nun* étant identique à un tracé de *taw* d'autant que, dans ces écritures, les traits des lettres ne sont pas toujours reliés entre eux, sans devoir y trouver à tout prix la preuve d'une relecture volontaire par le scribe qumranien en fonction de l'importance de la figure de

"How Old is the Aramaic Levi Document?," *DSD* 14 (2007): 291-312, qui fait clairement dépendre le *Testament de Lévi araméen* (son *Aramaic Levi Document*) de la source hébraïque des *Jubilés*, en le datant du dernier tiers du II^e siècle (299, 310 et *passim*). Par ailleurs, il me paraît douteux que le *Testament de Lévi* fasse l'éloge de la dynastie hasmonéenne.

(69) Voir R.A. Kugler, "Whose Scripture? Whose Community? Reflections on the Dead Sea Scrolls Then and Now, By Way of Aramaic Levi," *DSD* 15 (2008): 5-23, p. 10 ss.

(70) Kugler, *idem*, 9, notes 11 et 12, renvoie à des fragments où les variantes sont bien moins nombreuses qu'il ne paraît dans l'édition officielle.

(71) Voir Puech, *cit. RevQ* 80, 523, au lieu de *khwnwtk* de Kugler, *From Patriarch to Priest*, 80s. Kugler accepte maintenant ma lecture, *DSD* 15, 12 note 19: «confirms this reading». Greenfield-Stone-Eshel, *op. cit.*, 68-69 et 158, maintiennent leur lecture fautive de 4Q213b 1 1: le *bet* est assuré (départ de la tête [cuir plissé], coude et base partiellement effacée) mais le '*ain*' est exclu.

(72) Voir Kugler, *DSD* 15, *cit.*, 13-14.

Lévi dans la Communauté. Le copiste a pu avoir en mémoire le passage assez proche de *Jubilés* 30,7-18 et y avoir lu ou compris une parole de l'ange à Lévi, sans devoir trouver dans cette seule variante la preuve d'une relecture spécifiquement qumranienne. En effet, la copie ou une des copies originelles de la Geniza est certainement faite d'après un autre manuscrit provenant des grottes de Qumrân, et elle ne porte pas cette correction. La correction ne serait donc pas présente et reportée dans toutes les copies qumraniennes. (73) La lecture de 'l 'lywn me paraît certaine en 4Q213b, ligne 6, comme correction de 'lmy' ou 'lmy'n, et de même en Bodleian a 20 comme troisième étape de la correction. (74) Les erreurs et corrections de scribes ne sont pas rares dans toutes ces copies sans devoir toujours y chercher des relectures volontaires, d'autant que le passage précédent n'est pas préservé en 4Q213b qui, seul, permettrait de tirer une telle conclusion.

L'autre exemple avancé en ce sens est 4Q213a 3-4a dont la publication officielle laisse trop à désirer. (75) Mais alors que rien ne laisse supposer que ce passage était absent dans les copies de la Geniza et du grec, –les copies sont trop fragmentaires pour cela–, il ne peut être prouvé qu'il a été interpolé dans la copie araméenne pour servir les intérêts de la Communauté représentée par le chiffre codé Lévi! Kugler estime, sans preuve, que la section métrologique du grec est absente de l'araméen, alors qu'elle y est bel et bien attestée. (76) Il convient donc de rester très prudent avant de tirer de telles conclusions de recensions si différentes du *Testament de Lévi araméen* à Qumrân, types de copies certainement à la base des *Test. XII Patr.*, *Lévi*. Les lacunes des manuscrits ne peuvent entrer en ligne de compte pour bâtir des hypothèses. Ce serait une première avec des compositions araméennes à Qumrân, en dehors du *Targum de Job* qui paraît bien être une composition essénienne. (77)

(73) Dans l'état présent de la recherche, rien ne permet de distinguer deux recensions de cette composition araméenne. Les quelques variantes (oublis, corrections, vocabulaire, orthographe,...) ne sont pas plus importantes que celles qu'on lit dans le brouillon et la copie au propre de la *Supplique au gouverneur de Judée* par les juifs d'Éléphantine en 407, dus au même copiste, voir E. Sachau, *Aramäische Papyrus und Ostraka aus einer jüdischen Militär-Kolonie zu Elephantine* (Leipzig, 1911), Taf. 1-4.

(74) Voir Puech, *RevQ* 80, *cit.*, 523 et 525-26.

(75) Voir Puech, *DJD* XXXVII, *op. cit.*, 511-17. La présentation de Kugler, *DSD* 15, *cit.*, 15-16, doit être améliorée, comme j'ai pu le faire avec l'aide des seules photographies du PAM, mais il n'est pas le premier à lire *hsdh* à la ligne 6, Milik l'avait fait depuis longtemps, voir la *Concordance manuelle*, p. 2170 c. Son édition est loin d'avoir dit le dernier mot sur cette question.

(76) Kugler, *DSD* 15, *cit.*, 9-10 et 22. Dans ma note de la *RevQ* 80, 532, j'ai montré que 4Q214b 5-6 ii, correctement déchiffré, doit être identifié avec le passage de Bodleian d 12-18, lecture reprise avec raison par Drawnel, *op. cit.*, 132-33.

(77) Voir E. Puech, "Le Livre de Job à Qumrân," *V Convegno di studi biblici. Il Libro di Giobbe tradizione redazione teologia, 7-8 aprile 2006, Facoltà Teologica di Sicilia "San Giovanni Evangelista"* a cura di A. Passaro e G. Bellia (Palermo) (à paraître).

Ces *corrigenda* permettent de mieux comprendre le texte araméen conservé par la copie de la Geniza que recourent, par endroits, les restes de la copie grecque du Mont Athos et ceux des anciennes copies qumraniennes. Ils solutionnent une fois pour toutes l'attribution du passage de Camb. b à l'incident de Dinah.

Enfin, il est probable que Cambridge a-b a appartenu au troisième double folio et que le *Testament de Lévi* occupait plus que les cinq feuilles d'un livret habituel, Cambridge a pouvant être la neuvième colonne de la composition. (78)

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(78) Kugler, *From Patriarch to Priest*, *op. cit.*, 231-33, estimait l'original ne couvrir que trois feuilles, soit vingt-quatre colonnes.

A SCROLL IN ONE HAND AND A MATTOCK IN THE OTHER

A response

WE THANK Prof. Werrett for his helpful and insightful input and evaluation of our paper on the latrines at Qumran. In deed, his proposal at the end of his paper, taking a diachronic reading of the textual and archaeological evidence, is most helpful and serves to further collective discussion.

However, whereas Professor Werretts critique of our paper on the toilet practices of the Qumran community points out a minor error or two, he like many other textual scholars attempting to interpret the site, misread and misunderstand the Essene/Qumran hypothesis by focusing on but one or two items rather than the totality of the evidence. In his opening paragraph of the critique he quotes our 2006 article in which we state that the “new evidence bolsters the Essene hypothesis.” (1) Our deliberate choice of the phrasing “bolsters the hypothesis” as opposed to “proves” which we deliberately avoided should not be overlooked, something which is implied throughout the article. In the next paragraph he states that our theory became “in the eyes of many non-specialists, (2) an indisputable fact” whereas the article was peer reviewed by specialists before publication and, as we choose to publish it in a peer reviewed journal rather than submitting it to a non-peer reviewed popular journal speaks for itself. Our intent was not to prove nor disprove the Essene hypothesis, in fact, one of the co-authors has never been to the site, nor been involved in any Dead Sea Scroll/Essene research whatsoever. In fact, our earlier pa-

(1) Joe Zias, James D. Tabor and Stephanie Harter-Laiheugue, “Toilets at Qumran, the Essenes, and the Scrolls: New Anthropological Data and Old Theories,” *RevQ* 22/88 (2006): 631-40.

(2) As for ‘non-specialists’ reporting on the find in the popular media, it should be noted that the NY Times as well as Science and Nature ran the story in their journals.

per (3) on the latrine in locus 51, scholars use against the Essene is Qumran hypothesis, should in and of itself demonstrate that we have allowed the anthropological evidence to speak for itself, even when it may conflict with the Essene hypothesis, refutes his claim of having “pre-conceived notions” which have guided our research.

In our view Prof. Werrett confuses the main issues. First, it was not the case that we set out to “prove” the Essene hypothesis, and thus became guilty of circular argumentation. A scientific approach to archaeology never precludes reading textual and material evidence in conjunction, allowing one to interpret the other, so long as the material evidence is allowed full weight and is not shaped by a preconceived notion. We had no idea what might lie northwest of Qumran in terms of latrine evidence. For us to then survey the entire area, not just the northwest, and to discover the evidence we did, was a completely objective method of discovery and in no way prejudged what we would find, or might hope to find. Second, our method in no way undermined the clear differences in the genres of the various texts we drew upon (*Temple Scroll*, *War Scroll*, and *Josephus*). They do indeed differ in details, and as other scholars have concluded, they reflect diverse perspectives and points of reference. (4) However, our research demonstrated that their overall commonality was likely reflected in the latrine practices of those who lived in the settlement at Qumran.

His statement that we have not presented any artifacts or definitive archaeological evidence to support our claims, is, in and of itself misleading, if one reads our original two articles in *Revue de Qumran*. Not only have we presented new anthropological evidence in the way of species specific human parasites, aerial photographs but control samples within the plateau itself. Tabor and David Flusser had originally suggested that area to the northwest of the site may have been used as a latrine and taking Tabor’s advice we simply sampled the soils at random to determine if they were correct. They were both in fact correct.

Tabor suspected that there was visible surface evidence for what may have been a latrine or two however a subsequent visit by the author and aerial photographs led to the belief that the entire area, hidden from view of the ancient site was used as a latrine. Our fear that there would be animal contamination within the samples which can

(3) Stephanie Harter-Laiheugue, Francoise Bouchet, Kosta Mumchugulu, Joe Zias, “Toilet Practices Amongst Members of the Dead Sea Scrolls Sect at Qumran (100 BCE-68 CE),” *RevQ* 21/84 (2004): 579-85.

(4) “There was not just one theology at Qumran; the people who produced the scrolls had a range of ideas and beliefs on many topics, including messianism, purity, dualism and resurrection” Charlesworth, James H., ed. *The Bible and the Dead Sea Scrolls: The Second Princeton Symposium on Judaism and Christian Origins* (Waco, Tex.: Baylor University Press, 2006) 1: xxvii.

cause problems of identification for the parasitologist involved proved incorrect when non-human parasites were not present in any of the samples as samples were collected from below the surface.

Werretts's use of Hirschfeld's argument that locus 51 cannot be a toilet as there is no water channel for flushing cannot be taken seriously, as Hirschfeld appeared to be confusing latrines with public bath houses found in Caesarea, Beth Shean, and Hishams palace in nearby Jericho. One must remember that the *Temple Scroll* (11Q19 XLVI 13-16a) talks of roofed public latrines three thousand cubits from the city (Jerusalem) where excrement shall drop... And if one understands the city of Jerusalem one should be cognizant of the simple fact that there are no springs in that specific area. Furthermore, neither did the personal toilets discovered in the Roman villa atop Sepphoris nor the one in the City of David (Area G) have running water for flushing.

Contrary to the previous beliefs of some scholars, human specific parasites were found in locus 51 and non-human liver fluke parasites were found in the stables to the west thus confirming the original interpretations of De Vaux. Where human and animals were defecating in antiquity there will be parasitological evidence. To argue otherwise based on empirical evidence is beyond question.

Werrett argues that the finding of a toilet inside the site would call into question the association between Qumran and Essenes. However, as the parasites recovered in the sample can cause diarrhea, one may argue that it was used for emergencies as according to Rabbinical belief: "He who holds back his 'openings' transgresses the Biblical command: *you shall not make your souls detestable*" (Lev 20:25). (5) Furthermore, it should be clear that as the skeletal remains of one or two women were recovered in the cemetery toilet facilities in the barren wastes of Qumran would present innumerable problems for both sexes, therefore it is conceivable that this facility in L. 51 may occasionally have been intended for women. Furthermore there is the well known rabbinical belief that "no scholar should reside in a town without a public toilet." (6) Thus the finding of a small latrine within the site should not present the enormous and overstated difficulties which both Werrett and others have suggested. His statement that "one notes unwillingness on the part of Zias and Magness to acknowledge that the archaeological and textual evidence is difficult to reconcile" is unacceptable in light of the enormous amount of research particularly on the part of the latter, who on occasion disagrees with Zias' conclusions. (7)

(5) Julius Preuss, *Biblical and Talmudic Medicin* (F. Rosner ed.; London: J. Aranson and Co, 1993), 575. Also see *Numbers Rabbah* 16:24. "He who fails to heed the call of nature for four of five days will die."

(6) *Berakhot*, 61b, 62a, 28a; *Sanhedrin* 17b.

(7) Jodi Magness, "Toilet Practices at Qumran a Response," *RevQ* 22/86

The acknowledgment by the author that their earlier reading of scroll fragment (4Q472a) was erroneous is to be commended, particularly in the field of Qumran studies whereas Broshi earlier pointed out, in Qumran studies, no one admits to ones errors. (8)

Werrett's claim that the absence of any datable artifacts which could confirm the chronology of the latrines is clearly an argument along the lines of textual scholars with little or no field experience. Broshi in fact described the issue of the non-consensual theories more than a decade ago in that "all those theories are hardly ever held by more than one person, their progenitors" which some eleven years later still rings true. (9) What would one expect from the plateau in terms of datable artifacts? By excavating the plateau which Werrett calls for, what would one need to confirm or disconfirm the anthropological evidence which we have presented? These are not coprolites which one finds in caves, and C-14 dating the desiccated egg of a parasite is impossible. If we were fortunate to find one, there would not be any "signature" attesting to it being Essene. His suggestion that the Roman garrison may have used the latrine area following its abandonment in 68 A.D. is difficult to accept, as the Roman army had latrines and to suggest that the Romans would walk several hundred meters to defecate and then bury the fecal matter is hard to accept and is more in the realm of fantasy than reality. Moreover, liming the latrines for health reasons had, based on the City of David latrine, been known centuries earlier, thus for public health there was no reason to walk that distance to defecate. Same is true for the gentile world, which according to a recent paper, "relieving oneself in public, was one of the distinguishing features between Jew and non-Jew." (10)

Werrett concludes his paper by quoting Davies "When observation and theory become mixed up in the process of describing a site, the uninformed reader will likely be misled" (11) and that "Zias's theory has become virtually indistinguishable from observation." Here I would argue that Zias' theory for the Qumran is Essene hypothesis, is based not upon the latrine evidence alone, but the larger picture, particularly the evidence from the cemetery. Find another isolated site as other scholars have long suggested (12) on the western

(2006): 277-78. See reply Joe Zias, "Qumran Toilet Practices, A Response to a Response", *RevQ* 22/86 (2006): 479-481.

(8) Magen Broshi, "Was Qumran Indeed a Monastery?" in James H. Charlesworth (ed.) *Caves of Enlightenment*, (North Richardson, Texas 1998), 36.

(9) Broshi, M., *Ibid*.

(10) Zohar Amar, E. Baruch, "The Latrine (*Latrina*) in the Land of Israel in the Roman-Byzantine Period," in *Jerusalem and Eretz-Israel* (eds. J. Schwartz, A. Faust, Z. Amar, 2004) (Hebrew), English Abstract 5-6.

(11) Philip Davies, "How Not to do Archaeology: The Story of Qumran" *BA* 51/4 (1988): 206.

(12) Frank M. Cross, "The People of the Dead Sea Scrolls: Essenes or Saddu-

shore of the Dead Sea, with an adult male cemetery, no children, ten *miqvot*, three caves (7,8,9) on the plateau containing scrolls which can only be reached via the site, nine ink wells, the ostraca found by Professor Strange and recently republished by Puech (13) and I shall embark on the road to Damascus in search of the Essene settlement described by the ancients.

Lastly, the archaeological/anthropological evidence in support of the Qumran is Essene consensus stands despite all the alternative theories, when viewed in the light of the collective texts we have, from the Qumran scrolls themselves to Josephus on the Essenes, the correlation is remarkable, unequivocal and unmistakable.

Thanks to Professor James Tabor for his reviewing this response. All errors of interpretation as well as conclusions are the sole responsibility of the author.

Joe ZIAS

cees,” in *Understanding the Dead Sea Scrolls* (ed. H. Shanks; New York: Random House, 1992), 57.

(13) Emile Puech, “L’ostracon de Khirbet Qumrân (KhQ1996/1) et une vente de terrain à Jéricho, témoin de l’occupation essénienne à Qumrân, in *Flores Florentino: Dead Sea Scrolls and Other Early Jewish Studies in Honour of Florentino García Martínez*, (eds. A. Hilhorst, É. Puech and E. Tigchelaar, *Journal for the Studies of Judaism Supplements* 122; Brill: Leiden-Boston, 2007), 1-29.

RECENSIONS

M.G. ABEGG, with J.E. BOWLEY & E.M. COOK in consultation with E. TOV, *The Dead Sea Scrolls Concordance*. Volume One: *The Non-Biblical Texts from Qumran* [Part One and Two] (Leiden/Boston: Brill, 2003) xix+958 p. Relié Eur 300 - US\$ 225. ISBN 9004125213 DSC-001.

En 1960, K.G. Kuhn publia la *Konkordanz zu den Qumrantexten*, Göttingen, incluant les grands rouleaux hébreux alors connus, les manuscrits de la grotte 1 et quelques fragments des grottes 4 et 6; celle-ci rendit de très grands services, mais elle fut assez vite dépassée avec la publication d'autres fragments. Les éditeurs officiels avaient accès à un fichier-*Concordance manuelle* au Musée Rockefeller, un outil de travail en devenir constant selon l'avancée des travaux où était fichée la plupart des lectures. Ce fichier ne reflétait pas nécessairement l'état de la publication *princeps* à venir, fichier que, sans scrupule aucun, certains n'ont pas hésité à piller.

Avec ce volume de la *Concordance* en deux parties, les auteurs ont essayé de combler un besoin évident. Mais sans attendre la publication *princeps* de tous les manuscrits, contrairement à ce qui est affirmé dans l'introduction (1.1, p. ix), ce volume ne représente pas -et ne pouvait représenter- tous les textes non bibliques de Qumrân: hébreux, araméens, nabatéo-araméens et grecs. Et de fait, la *Concordance* reprend essentiellement les *indices* de la collection des *Discoveries in the Judaean Desert (of Jordan)*, à l'exception de quelques corrections typographiques, le *Document de Damas* (y compris les copies de la *Geniza* du Caire), *1QpHab*, *1QApGn*, *1QS-Sa-Sb*, *1QM*, *1QH*, *11Q19* (= RT), y compris les lemmes des *pesharim* et des fragments d'origine inconnue. Mais ne sont pas inclus les manuscrits cryptiques publiés en photographie sans transcription, ni les manuscrits des *Cantiques du sabbat* retrouvés à Masada pourtant repris en *DJD* XI (p. 239-52), ou le livre Deutéro-canonique de *Ben Sira* trouvé à Masada (mais il aurait fallu alors inclure aussi les copies de la *Geniza*), ou du *Testament de Lévi* araméen de la *Geniza*, alors que les copies parallèles de 1 et 4Q en soulignent la provenance évidente. Les auteurs ont fait un choix en faveur du seul *CD* et des ses parallèles qumraniens, choix qu'ils n'ont pas appliqué aux autres copies de compositions dans un même cas de situation, sans donner une explication.

Outre les mots d'usage commun, sont aussi répertoriés les noms propres. L'hébreu occupe le premier tome et la première moitié du second qui suivent l'araméen, puis le grec et, en appendice, les sigles des chiffres et quelques corrections d'erreurs typographiques. Sont aussi répertoriés des mots partiellement conservés, y compris par une seule lettre, et restaurés, mais en fait ce projet est très inégalement exécuté, avec bien des exceptions ou des restaurations plus qu'hypothétiques. Pour ce faire, les auteurs ont pris en compte, disent-ils, des rééditions de 1Q, 2Q et 6Q en *DJD* XXXVI, mais ces rééditions ne méritent pas bien souvent une telle appellation, tant elles sont à reprendre. L'ordre alphabétique des entrées n'est pas organisé selon les racines, comme dans les lexiques ou d'autres concordances, mais d'après l'orthographe, ainsi par exemple l'araméen *hymn*, qu'on ne doit pas chercher sous *'mn*. À côté de ces références de base, le renvoi au volume de publication en *DJD* est très utile, vu la dispersion insaisissable des manuscrits de la grotte 4, mais cette notation n'est pas exempte d'erreurs, par exemple les renvois de 4Q201 1 i-vi à *DJD* XXXVI où l'on cherchera en vain! Le renvoi aux colonnes et aux lignes est incompréhensible à propos de 1QH^a où les auteurs ont retenu ma mise en ordre du manuscrit mais ont conservé la numérotation des lignes de l'édition *princeps*. Or seule la colonne XVII échappe à des corrections, si bien que l'utilisateur est induit en erreur dans la presque totalité des citations qui vont hélas se répercuter longtemps dans bien des études ignorant la réalité de ce rouleau. Les utilisateurs ne pourront pas s'y retrouver, en attendant une publication ordonnée de ce rouleau.

Les auteurs n'ont pas fait un travail de révision et de contrôle approfondi des études parues depuis les éditions des textes, cela aurait demandé certes un très long et patient travail et retardé d'autant la parution, cependant ce labeur reste indispensable pour un produit de qualité et l'utilisation raisonnée de la *Concordance* qui ne peut être suivie aveuglément. De ce fait bien des mots fautifs sont répertoriés, alors que d'autres ont échappé, ainsi que de nouvelles identifications de fragments (voir 1.3, p. ix).

Comme une concordance est un outil indispensable pour l'étude des manuscrits, il est à souhaiter qu'une révision très approfondie en soit faite assez rapidement. Puis-je formuler un autre souhait? Ladite *Concordance manuelle* avait fort utilement réparti les entrées des verbes selon leurs conjugaisons: *qal*, *nif'al*, *pi'el*, *hif'il*, *hitpa'el*, de même pour l'araméen, dans les cas non douteux tout au moins. Serait-il possible de suivre cet exemple des pionniers? Pourquoi ne pas aussi adopter le système fort utile de la *Veteris Testamenti concordantiae hebraicae atque chaldaicae* de S. Mandelkern, Lipsiae 1896, qui permet de retrouver en un clin d'œil les formes grammaticales et orthographiques recherchées? Serait également utile en fin de chaque entrée alphabétique, ou en fin de volume, le répertoire des mots incomplets, répertoire qui pourrait aider à la poursuite du déchiffrement.

Après un début d'utilisation de ces volumes dans leur état présent, il apparaît que la *Concordance* ne peut être utilisée sans vérification dans de très nombreux cas. Cette mise en garde ne diminue pas le mérite des auteurs, mais elle voudrait les encourager à poursuivre afin de pouvoir disposer enfin d'un outil si utile et précieux, le plus complet et le plus exact possible.

Voici une liste de fautes relevées lors d'une première fréquentation avec des propositions de corrections qui peuvent être utiles à l'utilisateur, mais cette liste reste un échantillonnage parmi de nombreuses autres propositions:

P. 17b, *IQHa* XV 27 (non 24) supprimer la deuxième entrée et lire *b'd[y kwl]*, à reporter p. 154. P. 73b, *4Q171* 3-10 iv 4, lire '*l dbr(y)*' *h'mt*. P. 160a, *6Q15* 5 5, lire *twrt 'l* (p. 761a), non *lbryt 'l*. P. 178a, *4Q186* 1 i 9 (non 5) lire *sglglym* (p. 527a). P. 190a, *IQpHab* XII 15 lire *dwmh* à joindre au précédent. P. 243b, supprimer les deux attestations de *zydh* à lire *ndh* (p. 506b). P. 251b, *4Q251* 9 5, lire *hdšwt*. P. 276a, manque *4Q173* 5 5 *whrwt*]. P. 298a, *4Q377* 2 ii 5, ajouter *lkwl mš[wwt y]hwh*. P. 311b, *IQSa* II 11 est à lire '*m yw'd*]' [*hn*]' *š[y]*' *hmsyḥ*. P. 324a, *CD* XIII 1, corriger *mšyḥ*. P. 334b, supprimer l'entrée *k'yb*, et lire *k'wb* en *IQHa* XIII 30 (non 28) et XVI 29 (non 28). P. 349b, lire *mzl* (non *kwl*) en *IQHa* XVI 37 (non 36). P. 369b, *4Q171* 1-2 i 26 (avec M. Horgan, non 18). P. 388b, supprimer *krl* en *IQ22* 12 3, et lire *kwl*. P. 417b, outre les lignes qui ne correspondent pas à la restauration des colonnes du manuscrit de *IQHa*, comprendre *IQHa* XXIII 26 au lieu de 2 i 6. P. 433b, ajouter *IQpHab* VIII 17 *wh*]*m[y]r ḥwqyl*['*l*. P. 438b et p. 595a, ajouter *IQSb* IV 23, *l'* *šwt lmḥbrt bydkh*. P. 473a, ajouter *4Q171* 1-10 ii 11 *b*]*kwl m*['*d*]*ny h'rš*. P. 482a, ajouter *4Q377* 2 ii 5 *lkwl mš[wwt y]hwh*. P. 531a-b, en *4Q177* 1-4 10 lire *wnsłḥ lhm* au lieu de *wnsłw*, et ajouter *4Q512* XII 4 *lslyḥwt 'wl*]. P. 555b en *1IQ19* XLVI 1, lire *wlw*]' *yškw*n [au lieu de *y'wp*. Pp. 609a-610b, changer l'impossible *pwt*]'ys en *pytl*]'ws "Peitolaos". P. 636a, *4Q525* 15 7, comprendre *šwny šḥt* "les pièges de la fosse". P. 689a, en *4Q504* 1-2 iv 7 lire *mzr'w* au lieu de *kr'y* douteux. P. 694a, en *4Q215a* 1 ii 4 lire *pš'm* au lieu de *rš'm* (p. 626a). P. 503a, en *IQ29* 13 3, lire *wšqr gwh* (ajouter p. 750b). P. 705b, ajouter *4Q418* 69 ii 6 [*š*]'wl. P. 722a, corriger *šwt* en *šyt* en *4Q417* 1 i 15. P. 747b, en *IQM* V 10, lire *wšph* au lieu de *wšpwd* et en *CD* X 18, lire *yšpwṭ* au lieu de *yšpwkw*. P. 756b, ajouter *4Q174* 1-2 i 7 *twdh* au lieu de *twrh*, et p. 761a, ajouter *twrt 'l blbbm*. P. 763a, en *4Q415* 11 9 lire *tkwnh* au lieu de *tkynh*. P. 767b, en *4Q171* 1-2 ii 10, lire *ht'nyt* et de même en *4Q171* 1+3-4 iii 3, *h*]*t'n*]*yt* (ajouter p. 768a) au lieu de *h*]*t'*]*wt*.

Pour la partie araméenne des manuscrits *4Q550* à *575a* et *580* à *587*, je renvoie aux *indices* de *DJD* XXXVII, tant de nombreuses références et des lectures doivent être revues, et des entrées font défaut. P. 782b et 784b, en *4Q213a* 3-4 18 corriger en *l' l mn* au lieu de *l' lpn*. P. 794a, *4Q201* 1 i-vi n'est certainement pas publié en *DJD* XXXVI. P. 804b, ajouter *4Q213a* 3 15 *btw*]*lh zy*, et pour ce fragment en général, voir 'Appendices' de *DJD* XXXVII, tant l'édition de *4Q213-214b* doit être sérieusement revue. P. 824a et 921b, ajouter *4Q213b* 1 1 *hy*]*kh rbytk mn kl bsr*]' au lieu de *kh r'yt*k... (p. 925a). P. 829b, ajouter *4Q196* 3 10 (= Tb 3,15) *wl*]' *h*]*bl šm*]*y wšm 'b*]*y*. P. 831a, les entrées de *4Q545* à *1IQ10* *sub ḥdh* sont imprimées en romain. P. 834a, 897a et 917a, en *4Q201* 1 iv 8 lire *dy qtyly*' au lieu de *dy* [*t*]*ḥty* '*lyh*. P. 834b, en *4Q243* 11 ii 1, lire *whk*]*y**my*]*n* au lieu de *whk*]*ny*]*n*. P. 837a, en *4Q213a* 3 16 corriger l'impossible *ḥsyh* en *ḥsdh*. P. 845, *yldy* de *4Q203* 7b i 3 est certainement exclu, lecture probable *hly*' "les malades". P. 847a, ajouter *4Q211* 1 i 3 [*šb* '*r*'' '*y*''', à corriger p. 903b. Pp. 851a et 904a, corriger *kw*]*kby šmy*' en '*rby šmy*' en *4Q209* 23 4. P. 864a, ajouter *4Q213a* 3 17 [*l*]*y*]*t lkl dry* '*lm*'. P. 869b, ajouter l'entrée *mzg*' de *bty* [*m*]*zy*]*g*' en *5Q15* 1 ii 10. P. 871a, corriger *wmn mly* [*yryn*] de *4Q201* 1 i 3 en *wmn mmr* [, ajouter p. 867. P. 875a et 900b, en *4Q246* 1 i 2 lire *{l}*]*«b»* '*lm*'. P. 875b, en *4Q318* VIII 9, lire *mn bry*' au lieu de *mnkry*', mais *mby*]'*l* en *4Q339* est hébreu, non araméen. P. 884a, en *4Q206* 4 ii 6 lire *ypl**twn* au lieu de *wnḥtyn*.

P. 886b, rattacher *nšbh* de *11Q10* XXVI 5 au suivant p. 887a. P. 890b, ajouter *4Q231a* 3 14 '*lyh lms[r]ph*'. P. 903b, ajouter *6Q8* 3 2 *kl* '«'» *gnth* et supprimer *4Q211* 1 i 3. P. 906a et 797b, en *4Q206* 3 i 5 [= 206a], lire *jm* «*mn b'st*» au lieu de *jm mt'st*. P. 918a, *4Q201* 1 i 7 à lire deux fois *qšwly* et *qšw[y]* au lieu de *qšwt*. P. 925b, *1Q20* XX 28 lire '*lwhy b'dn*' au lieu de '*l[d]y [yt]rp*'. P. 958b la faute typographique de l'édition est faussement corrigée, lire *wlb[nyhwn]*.

É. PUECH

L. DiTOMMASO, *The Dead Sea New Jerusalem Text. Contents and Contexts, Texts and Studies in Ancient Judaism/Texte und Studien zum Antiken Judentum*, 110. xv + 228 p. Mohr Siebeck, Tübingen 2005. Relié Eur. 79, ISBN 3-16-148799-0, ISSN 0721-8753.

Ce livre est la version révisée de la thèse de doctorat de l'auteur au Département des Religious Studies à McMaster University en 2001. L'auteur se propose d'étudier la composition de la *Jérusalem Nouvelle* (= *JN*) en araméen, thème biblique déjà connu en Ézéchiel et par l'*Apocalypse* de Jean (avons-nous affaire à une Jérusalem restaurée ou idéale, une cité terrestre ou céleste?), et enfin de comprendre la nature de cette œuvre. Mais auparavant il est indispensable d'étudier de près les fragments conservés en *1Q32*, *2Q24*, *5Q15* et *11Q18* parmi les manuscrits pour lesquels on dispose de leur *editio princeps*, ce qu'il ne fait jamais, et pourtant il y a matière à révision, mais il faut être formé pour cela. Pour les fragments de la grotte 4 dont la publication était à la charge de J. Starcky qui m'en transmet les droits, l'auteur pense faire œuvre d'éditeur en présentant une 'working edition' (p. 9) des manuscrits *4Q554*, *4Q554a* et *4Q555* d'après la numérotation que j'en ai faite, y compris une complète reconstruction du texte de *4Q554* 1 i-ii + *4Q554a* (par. *2Q24* 1; *5Q15* 1 i-ii) accompagnée d'une traduction et de notes de lecture. On a du mal à comprendre qu'une telle 'édition' puisse passer pour un 'desideratum' (p. 9), outrepassant, qui plus est, les droits de publication.

L'étude prétend prouver cinq points: 1) que la distinction *4Q554* et *4Q554a* est correcte, 2) que *4Q554* 2 iii doit inclure le fragment 6, 3) que *4Q554* 7 appartient à *4Q554* 2 iii 19-22, 4) que les mesures des murs de la *JN* (100 x 140 stades) est incorrecte et 5) que les noms des portes manquant en *4Q554* 1 i 11 - ii 10 ne peuvent être restaurés automatiquement à l'aide des listes de *RT* et de *Reworked Pentateuch* [p. 10 n. 34, l'auteur ignore mon étude de 1995 citée dans celle de 2003, et où les noms des portes de *JN* étaient déjà donnés]. Mais les points 1) et 5) étaient déjà résolus par leur éditeur officiel et les autres sont de fausses interrogations avec de fausses réponses, voir ci-dessous. (Dans cette publication, le lecteur doit savoir que les numérotations des fragments sont celles propres à l'auteur, ce qui ne facilite pas les choses, bien au contraire mais qu'on pourra vite oublier, non celles de l'édition officielle).

Le premier chapitre présente les fragments de la grotte 4. L'auteur estime assuré le joint du fragment de la bataille eschatologique (p. 14) avec la feuille précédente. Le fragment étant enserré dans un cadre pour son exposi-

tion au musée, il m'a été impossible de le vérifier manuellement pas même visuellement; or comme les diverses photographies ne montrent pas que le fil soit coupé ou que les trous de la couture soient abîmés, les fragments auraient dû être trouvés attachés côte à côte dès le début, ce qui n'est manifestement pas le cas. En conséquence, dans le doute, j'ai cru bon d'attribuer un numéro différent à ce fragment dans l'*editio princeps*. En revanche, les fragments que l'auteur a cru identifier n'appartiennent pas à ces rouleaux. Ainsi 4Q554 7, correctement lu, est identifié à 4QDn^a 2,44-46 (*DJD* XVI p. 247) et 4Q554 8 à 4QDn^a 2,36-39. Quant à 4Q555, l'auteur pense fièrement avoir identifié trois nouveaux fragments, or seuls les trois fragments identifiés par Starcky sont à retenir, auxquels il faut y joindre un autre, mais inconnu de DiTommaso. Il va sans dire qu'un grand nombre de lectures et de restaurations ne sont pas à retenir. Mais il est inutile de les relever ici, le lecteur se reportera à l'*editio princeps* en *DJD* XXXVII (non XXXVI, p. 7 n. 20). L'auteur revient longuement et à plusieurs reprises sur des fausses lectures des mesures par Starcky, confondant les chiffres 5 avec 4, 8 avec 7, or le scribe a bel et bien écrit 25 et 18 avec Starcky, non 24 et 17, comme il l'affirme avec entêtement (p. 30s, 60s, 77 ss, etc.). Là encore, sa compétence paléographique y est sérieusement mise en question, or elle est essentielle pour un déchiffreur et une édition digne de ce nom.

Le deuxième chapitre donne la traduction des principaux fragments de *JN*. La remise en ordre vaut ce que valent les lectures, les restaurations et les identifications (en particulier le fréquent renvoi au 'faux' 4Q554 7). L'auteur me reproche (p. 104s, n. 44) l'utilisation des termes *decumanus* et *cardo*, car toute influence romaine est exclue, —ce que je n'ai jamais dit ni revendiqué—, mais le sens de ces termes dans un plan de ville est compris de tous, autant et mieux que ceux d'avenue, boulevard, rue et ruelle qui ne répondent pas davantage à un plan hippodamique. Par ailleurs, l'auteur attribue à E. Tov ce qui revient à A. Lange (p. 110 n. 73, n. 78). Même si une source est un élément important dans une ville, *JN* ne porte aucune mention de source dans les parties conservées (p. 119s, 4Q554 4, p. 69, est encore une fausse lecture), seul 4Q537 fait allusion à ce thème de l'eau, en dehors d'*Ézéchiél* 47. L'auteur écrit que *JN* doit être considérée comme une apocalypse, et que la Nouvelle Jérusalem ne doit pas être comprise comme la Jérusalem céleste d'après celle du Nouveau Testament, mais qu'elle doit se situer entre une Jérusalem terrestre restaurée ou rénovée dans le futur et un type monumental de la *JN* (p. 129 ss). Qu'est-ce à dire en réalité? À ce propos, il aurait été utile de noter que le périmètre de la ville et la hauteur des murs sont comparables à ceux des descriptions de Ninive et de Babylone (voir *DJD* XXXVII).

Le troisième chapitre passe en revue les rapports possibles entre *JN* et d'autres manuscrits de Qumrân, et en premier lieu le *Rouleau du Temple* avec lequel il a peu en commun en matière de genre, de forme et de contenu. L'auteur relève quinze points de différence, à commencer par l'araméen, à la troisième personne, contrairement à l'hébreu et la première personne de *RT*, etc., pour conclure qu'il y a peu de correspondances entre les deux compositions et que celles-ci ne dépendent pas l'une de l'autre ni d'une source commune (p. 158s). Contrairement à ce que l'auteur m'attribue (p. 145 n. 45), je n'ai pas écrit que 4Q365a "might represent a very old copy of the Temple Scroll", mais qu'« on ne peut affirmer sans quelque précaution que 4Q365a

est la plus ancienne copie de RT. Tout au plus serait-on autorisé à dire que ces fragments contiennent des passages d'une source possible... », ce qui n'est pas du tout la même chose! De même, il n'a pas compris le point de vue de Stegemann qui n'a pas écrit que les manuscrits de la grotte 1 ont été déposés 'a short time after June of 68 CE' (p. 186), mais qu'ils ont été déposés 'avant la chute de Qumrân puisque très peu après le 21 juin Qumrân a été détruit' (voir Stegemann, *op. cit.* p. 93). En ce qui concerne 4Q391, 4Q462, 4Q537, 4Q529 [auquel il faut ajouter maintenant 4Q571, identification assurée quand paraissait *DJD XXXI*, mais les lectures, p. 166 n. 79, sont à revoir] et 4Q475, il y a peu de rapprochements à retenir, car trop souvent l'auteur se fonde sur des éditions trop défectueuses dont il n'est pas à même d'apprécier la valeur, ainsi en est-il par exemple de l'édition de 4Q475 qui est à revoir entièrement.

L'auteur s'intéresse ensuite à JN dans le contexte eschatologique de la communauté qumranienne, mais qu'on doit apprécier sans le (faux) fragment 4Q554 7. On a affaire, dit-il, à deux listes dans un schéma des quatre royaumes, comme en Daniel araméen. L'auteur est d'avis que JN n'attend pas de bataille eschatologique (p. 173 ss, mais contredisant alors ce qu'il écrit à la page 14!). Pourtant il me paraît assuré que le manuscrit attend cette victoire du peuple/royaume de Dieu à la fin. Pour DiTommaso, JN est une apocalypse qui requiert un horizon eschatologique et la nouvelle Jérusalem restaurée doit y rassembler tout Israël à la fin des temps, avec des rues pour le trafic et le commerce, etc. Mais alors l'auteur ne se pose même pas la question de la source ou origine de ce commerce ni de l'approvisionnement du temple: les nations étrangères? Nulle part en JN, Dieu est dit créer une structure eschatologique qui ne serait ni préexistante ni céleste, et, puisqu'il y a peu en commun entre les deux compositions (voir ci-dessus), on ne peut reporter en JN la conception de RT XXIX 8-10: «...Je consacrerai pour ma gloire mon sanctuaire sur lequel je ferai demeurer ma gloire jusqu'au jour béni [non de la création, qui est une lecture impossible] où je créerai moi-même mon sanctuaire pour tous les temps... » (p. 179). Et malgré un horizon eschatologique assez proche de ceux de RT, 4QFlorilège et 1QM, il y a peu d'éléments de contacts dans l'architecture ou les attentes eschatologiques, surtout pas de bataille finale, précise-t-il encore une fois (p. 184). Malgré les sept copies retrouvées, une origine qumranienne est exclue —ce qui paraît raisonnable—, mais est-il si clair pour autant que les Qumraniens attendaient "a discreet Jerusalem and/or New Temple" (p. 189)? Enfin, l'auteur estime que toutes les copies datent de l'époque hérodienne. Mais j'ai daté 4Q554a de préférence de l'époque hasmonéenne. L'auteur date la composition dans le premier tiers du II^e s. avant la crise maccabéenne, en réponse à une situation politique, sociale et religieuse de l'époque, ceci paraît probable.

A la fin de la lecture de cette supposée "working edition", où se trouvent, entre autres, tant d'erreurs de déchiffrement, d'identification et de références, sans parler des contresens dans la lecture de la littérature en langue étrangère, erreurs qu'un lecteur attentif repérera aisément, on se pose la question de l'utilité de cette publication, ainsi que l'écrivait déjà E. Tigchelaar dans sa recension du livre de M. Chyutin (*RevQ* 71 [1998] 457). Elle ne peut même pas être qualifiée d'édition préliminaire. Le lecteur se devait d'en être informé.

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TABLES DU TOME XXIII

AUTEURS CITÉS

- Abegg (M.G.) 109; 173; 194;
338; 339; 377; 431; 569-572
Abel (F.M.) 418; 424
Abu-Rabia (A.) 486
Abusch (R.) 353
Achtemeier (P.J.) 109
Adamczewski (B.) 109
Adamczewski (B.) 431
Adams (M.P.) 575
Adler (W.) 575
Adler (Y.) **79-92**
Adler (Y.) 271
Aejmelaeus (A.) 462; 467; 471
Aharoni (Y.) 65; 66; 80; 89
Aitken (J.K.) 575
Albani (M.) 431
Albeck (H.) 516; 519
Albright (W.F.) 397
Albright (W.F.) 65; 418
Alexander (Ph.) 74; 109; 250;
257; 271; 311; 353; 378
Allen (J.) 431
Allen (J.S.) 575
Alon (A.) 419
Aloni (N.) 349
Alonso Fontela (C.) 271
Al-Rawi (F.) 22; 27; 29; 36s; 38
Altmann (A.) 352
Amar (Z.) 566
Amara (D.) 470
Amaru (B.H.) 109
Ameling (W.) 109
Amiran (D.) 416; 424
Amit (D.) 358
Amitay (O.) 431
Amusin (J.) 65
Anderson (A.A.) 102
Anderson (J.S.) 271
Andreasen (N.E.) 109
Angel (A.R.) 271
Antonissen (H.) 431; 575
Arbel (D.V.) 271
Arcari (L.) 271; 272; 575s
Arnold (R.C.D.) 272; 344
Ashley (T.R.) 65
Ashton (J.) 431
Assefa (D.) 431
Asurmendi Ruiz (J.) 109; 272
Atkinson (K.) 68; 70; 110; 272;
331; 332; 431; 576
Attridge (H.W.) 110
Aune (D.) 432
Ausín (S.) 110
Ausloos (H.) 272; 467
Auwers (J.-M.) 272; 432
Avermarie (F.) 576; 110
Avery-Peck (A.J.) 356
Aviam (M.) 272
Avigad (N.) 403
Avioz (M.) 576; 110
Avi-Yonah (M.) 423; 426
Baarda (T.) 546
Baasten (M.F.G.) 272
Bachmann (M.) 273
Bacon (B.) 340
Baer (D.A.) 110
Baillet (M.) 55; 80; 88; 338; 529

- Balentine (S.E.) 576
 Baly (D.) 418
 Bande García (J.A.) 110
 Baraniak (M.) 576
 Bar-Asher (M.) 110; 432; **491-524**; 576
 Barclay (J.) 111; 273; 432
 Bar-Gal (G.K.) 110
 Barkai (G.) 358
 Barker (M.) 577
 Barker (M.) 111
 Bar-Kochva (B.) 69; 432
 Barr (J.) 467; 111
 Barstad (H.M.) 378
 Barthélemy (D.) 80; 410; 413; 471; 406s
 Baruch (E.) 566
 Baruch (U.) 422
 Baruchi (Y.) 85
 Barzilai (G.) 273
 Baslez (M.-F.) 111; 273; 432
 Batsch (Ch.) 111; 432; 577
 Bauckham (R.) 111; 577; 488
 Baumgarten (A.I.) 155; 273; 432; 481
 Baumgarten (J.M.) 111; 187; 194; 234; 241; 244; 253; 268; 273; 347; 362; 366
 Baumgarten (W.) 47; 71; 396
 Bautch (K.) 577; 273; 274
 Baxter (W.) 111
 Bazlez (M.-F.) 274
 Bearman (G.) 403
 Be-Avraham (Z.) 420
 Becker (E.-M.) 274
 Becker (H.) 317
 Becker (M.) 335; 336s; 432
 Becking (B.) 342
 Beentjes (P.C.) 111; 208; 274; 432s; 577
 Begg (Ch.T.) 111; 433; 577
 Beit-Arié (M.) 547
 Bekkum (W.J. van) **341-356**
 Bellia (G.) 560; 274
 Bender (M.) 421
 Ben-Dov (J.); 112; 419; 433; 577
 Ben-Hayyim (Z.) 501; 519
 Benoit (H.) 80
 Ben-Yair (C.) 112; 335; 336s
 Bergsma (J.S.) 274; 577
 Berlejung (A.) 433
 Berlin (A.) 211; 346
 Berner (Ch.) 577
 Bennett (M.) 274; 433
 Bernstein (M.) 112; 158; 178; 254; 391; 400; 429s; 578; 433
 Berrin (Sh.) 65; 434
 Berthelot (K.) 112; 274; 578
 Betz (O.) 240
 Beyer (K.) 15; 20; 112; 543; 547; 549
 Beyerle (S.) 274; 578
 Bhayro (S.) 20; 112; 275;
 Bickerman (E.J.) 434
 Biguzzi (G.) 434
 Bilde (P.) 112
 Bird (M.) 275; 578
 Birenboim (H.) **357-369**
 Birkan-Shear (A.) 275
 Black (M.) 12; 17; 38; 211
 Blanton (W.) 578
 Blasius (A.) 112
 Blau (L.) 318; 321
 Blenkinsopp (J.) 238; 434
 Bloch (M.) 578
 Bloom (M.) 578
 Boccaccini (G.) 112; 275; 385s; 434; 578
 Bockmuehl (R.) 579
 Boda (M.) 434; 112
 Bohak (G.) 579
 Bolyki (J.) 275
 Bond (H.) 434; 579
 Bons (E.) 275
 Borbone (P.G.) 405
 Bord (L.-J.) 113
 Boring (M.) 434
 Borukhov (E.) 275
 Botha (P.J.J.) 579
 Bottema (S.) 424
 Böttrich (Ch.) 434
 Bouchet (F.) 564
 Boustan (R.) 275; 434
 Boyarin (D.) 579
 Brack-Bernesen (L.) 38; 30
 Braudel (F.) 416; 419
 Bredin (M.) 113
 Bremmer (J.N.) 546
 Brettler (M.) 461
 Breuer (Y.) 519; 498

- Briggs (C.A.) 47
 Brizemeure (D.) 275
 Brockelmann (C.) 20; 39
 Brody (R.) 346
 Brogan (T.V.F.) 225
 Bromiley (G.W.) 71
 Brooke (A.E.) 405
 Brooke (G.) 43; 88; 89; 114; 154; 157; 239; 275; 276; 365; 434; 472
 Broshi (M.) 114; 276; 426; 531; 566
 Brown (D.) 26
 Brown (F.) 47
 Brown (J.) 39; 114
 Brownlee (W.H.) 71; 383
 Brutti (M.) 435
 Bryson (R.) 423; 425
 Budd (Ph.) 71
 Bunta (S.) 114; 276
 Burkes (Sh.) 114
 Burns (J.E.) 276; 579
 Busch (P.) 435
 Busto Saiz (J.R.) 405
 Butf (S.) 115
 Butterweck (A.) 352
 Buttgieg (D.) 459
 Butzer (K.) 419s

 Calduch-Benages (N.) 115; 276
 Callaway (Ph.) 435; 115
 Cambiaso (C.) 405
 Camp (C.) 276
 Campbell (G.) 579
 Campbell (J.) 85
 Canonica (L.) 115
 Capper (B.J.) 115; 276
 Capponi (L.) 239; 579
 Carbullanco Núñez (P.C.) 435
 Carey (G.) 579
 Carmi (I.) 421
 Carmignac (J.) 242; 244; 376; 378
 Carpenter (M.) 345; 351
 Carrez (M.) 399
 Carson (D.) 435
 Castelli (S.) 115; 369
 Catastini (A.) 407; 579
 Chae (Y.S.) 579
 Chalcraft (D.J.) 435
 Chambon (A.) 480; 485
 Chancey (M.A.) 276
 Chapman (D.W.) 115
 Chapman (H.H.) 276
 Charles (R.H.) 65; 115; 116; 159; 240s; 276; 277; 352; 353; 463; 545; 564; 579
 Charlesworth (J.H.) 69
 Chazon (E.) 44; 55; 158; 255; 256; 257; 277; 343; 344; 348; 352; 356; 383; 435
 Cherian (J.) 116
 Chesnutt (R.D.) 116
 Chester (A.) 277
 Chialà (S.) 579
 Choi (P.R.) 116
 Christian (M.A.) 277
 Cicieliag (J.) 278
 Clanton (D.) 580
 Clements (R.) 44; 116; **109-149**; 268; **271-306** ; 278; 344; **431-456**; 580
 Clifford (H.) 435
 Clifford (R.J.) 278
 Clines (D.) 47; 234
 Cogan (M.) 397
 Coggins (R.) 435
 Cohen (H.) 491; 507; 513
 Cohen (N.) 435
 Cohen (N.G.) 116
 Cohen (Sh.) 81; 155; 278
 Cohn (Y.) 83; 580
 Cohn Eskenazi (T.) 429
 Collins (J.); 64; 67; 68; 73; 74; 117; 208; 228; 252; 253; 258; 278; **309-311** ; 314; 325; 336; 336s; 338; 339; 340; 436; 580
 Collins (M.A.) 436
 Collins (T.) 212
 Conklin (B.W.) 278
 Conrad (J.) 234
 Cook (E.) 117; 173; 569-572
 Cook (J.) 278; 378; 436
 Corley (J.) 117; 278; 436
 Costa (T.) 580
 Cotton (H.M.) 117; 493; 504; 520
 Coulot (C.) 580
 Cousland (J.R.C.) 580
 Coward (K.) 381
 Cowley (J.E.) 569-572

- Cox (C.E.) 466; 467; 377
 Craven (T.) 117
 Crenshaw (J.L.) 462
 Cross (F.M.) 69; 84; 86; 93; 117;
 217; 267; 279; 309; 328; 412;
 466; 526; 566
 Crown (A.) 419
 Culpepper (R.A.) 117

 Dąbrowa (E.) 436
 Dahmen (U.) 117; 342; 344; 436;
 461; 463
 Danino (Sh.) 436
 Dart (J.) 580
 Davidson (I.) 343
 Davies (Ph.R.) 118; 252; 436;
 477; 489; 566; 580
 Davila (J.) 118; 279; 353; 580
 Davis (C.J.P.) 118
 Davis (M.) 581
 Day (J.) 279
 Day (P.) 162
 Crom (D. de) 581
 Jonge (M. de) 69; 70; 552; 545s
 Lange (N.R.M. de) 118
 Roo (J.C.R. de) 377
 Troyer (K. de) 168; 279
 De Vaux (R.) 80; 485; 529
 Debel (H.) 220; **459-473**
 Deconick (A.D.) 279
 Deines (R.) 436s
 Del Olmo Lete (G.) 20; 39
 Delcor (M.) 208; 209; 217; 218;
 219; 222; 331; 461
 Delfes (H.) 423
 Deroo (J.C.R.) 194
 Desilva (D.A.) 437
 Desilva (M.) 118
 Desnica (V.) 118
 Deutsch (C.) 208; 215; 217; 218;
 219; 220; 222; 228
 Di Benedetto (A.) 405
 Di Lella (A.A.) 74; 208; 209;
 213; 215; 221; 225; 229; 279
 Dietrich (W.) 279
 Díez Fernández (F.) 118
 Dihi (H.) 581
 Dimant (D.) **43-61** ; 119; 160;
 194; **233-245** ; 279; 311; 335;
 339; 364; 383; 384; 386s; 437;
 491; 509; 510; 520

 Ditommaso (L.) 279; 280; 437;
 572-574; 581;
 Dobbeler (S.) 437
 Doehorn (J.) 280; 581
 Dodson (J.R.) 437
 Doering (L.) 119; 437; 582
 Dogniez (C.) 119
 Domb (A.J.) 119
 Dombrowski (B.W.W.) 437
 Donaldson (T.L.) 119
 Donceel (R.) 119; 280
 Donfried (K.P.) 280
 Doran (R.) 120; 582
 Doré (J.) 399
 Dorival (G.) 280
 Dorman (J.H.W.) 280
 Doudna (G.) 120
 Drawnel (H.) **3-41** ; 120; 388;
 437; 544; 549; 582
 Dreyfus (F.) 399s
 Driver (S.R.) 47; 396; 407
 Dubs (J.-C.) 120
 Dudley Stamp (L.) 419
 Duhaime (J.) 248; 249; 280; 582
 Duke (R.) 280
 Duncan (J.A.) 527
 Dunn (J.D.G.) 120
 Dupont-Sommer (A.) 465s; 472
 Dušek (J.) 319; 437

 Edrei (A.) 280; 582
 Egger-Wenzel (R.) 280
 Egmond (R.) 280
 Ego (B.) 280; 281; 356
 Ego (B.) 437; 582
 Ehrenkrook (J.) 582
 Ehrlich (A.B.) 520; 509
 Eichler (B.L.) 397
 Eisenman (R.) 582; 120
 Elbogen (I.) 314; 316; 317; 318;
 321; 323; 333
 Elgvin (T.) 120; **261-268**; 356;
 385s; 484; 582
 Eliav (Y.Z.) 281
 Elior (R.) 194; 281; 583
 Elitzur (B.) 491ss; 343
 Elledge (C.D.) 281
 Elliott (J.K.) 120
 Elwolde (J.) 163; 215; 342; 438
 Embry (B.) 281
 Emerton (J.A.) 159

- Emery (K.O.) 419
 Endres (J.C.) 120; 281
 Epstein (J.N.) 82; 504
 Erder (Y.) 438
 Eshel (E.) 69; 88; 89; 261; 281;
 339; 356; 388; 531; 543ss; 583;
 438
 Eshel (H.) 69; 79; 121; 281; 358;
 438; 583
 Eskhult (M.) 583
 Estes (D.) 282
 Evans (C.) 121; 282; 338; 463;
 438
 Exum (J.C.) 466
 Eybers (I.H.) 412
 Eynde (S.) 121; 438;
 Eynicke (E.) 51

 Fabry (H.-J.) 121; 282; 438; 463;
 583
 Falk (D.) 121; 348; 583; 438
 Fassbeck (G.) 282
 Fassberg (S.) 121; 282; 583
 Faust (A.) 566
 Feldman (A.) 438s
 Feldman (L.) 121; 282; 439
 Fermor (J.) 26; 39
 Fernández Lago (J.) 121; 122;
 282; 405; 466; 468; 471
 Fernández Sangrador (J.J.) 122
 Fidler (R.) 439
 Fields (W.W.) 282
 Filed (W.W.) 44
 Filson (F.) 418
 Fincke (A.) 97; 99; 101; 409
 Findlay (J.D.) 439
 Fine (S.) 354
 Finkelstein (L.) 323; 461
 Fisher (L.R.) 363
 Fisher (Th.) 327
 Fisk (B.) 439
 Fitzgerald (A.) 233
 Fitzmyer (J.) 347; 439; 583
 Flannery-Dailey (F.) 282
 Fletcher-Louis (C.) 122; 283
 Flint (P.W.) 122; 212; 338; 342;
 347; 366; 439; 460; 462; 463;
 464; 527; 583
 Floyd (M.) 122
 Flusser (D.) 64; 69; 180; 314;
 331; 335; 564; 583

 Fonrobert (Ch.E.) 283
 Förster (N.) 283
 Foster (P.) 122; 283; 439; 584
 Fox (M.V.) 84
 Fraade (S.) 122; 268; 283; 439
 Fraile Yécora (P.) 122
 Frank (H.Th.) 283
 Frankel (Y.) 399
 Freedman (A.D.) 584
 Freedman (D.N.) 65; 122; 103;
 153; 211; 384; 416
 Frerichs (E.S.) 471
 Frey (J.) 254; 123
 Frey (S.) 439s; 270
 Fried (L.S.) 440
 Friedman (Th.) 162
 Fröhlich (I.) 584
 Frulla (G.) 584
 Frumkin (A.) 421s
 Fuchs (C.) 283
 Fuller (M.) 440; 123
 Fullmer (P.) 584

 Gafni (I.) 69
 Gagné (A.) 584
 Galimberty (A.) 283
 Gambetti (S.) 584
 García Martínez (F.) 4; 7; 17; 39;
 123; 159; 171; 173; 174; 181;
 184; 186; 203; 241; 248; 254;
 284; 336s; 342; 353; 366; 372;
 378; 381; **383-394**; 430; 440;
 459; 529s; 546; 584
 Gardner (G.) 440
 Garnet (P.) 123
 Gärtner (B.) 239
 Gathercole (S.) 123; 584
 Gauger (J.-D.) 285
 Gazor-Ginzberg (M.) 74
 Geiger (A.) 406
 Gemünden (P. Von) 285
 Gentry (P.J.) 466; 467
 George (A.R.) 22; 27; 29; 36s; 38
 Gerber (Ch.) 440
 Gerstenberger (E.) 584
 Gibson (Sh.) 123
 Gieschen (Ch.A.) 585
 Gilbert (M.) 285
 Gilders (W.) 440
 Gillingham (S.) 123
 Ginsberg (H.L.) 72

- Ginzberg (L.) 363
 Ginzburg (Ch.D.) 411
 Gitin (S.) 237
 Giulea (D.-A.) 440
 Glielich (A.) 285
 Glinert (L.) 440
 Glueck (N.) 418
 Gmirkin (R.) 69
 Gmirkin (R.) 124
 Goff (M.) 585
 Goff (M.J.) 255; 285; 377; 378; 381
 Goldenberg (D.M.) 285
 Goldingay (J.) 73
 Goldman (L.) 124; 440
 Goldman (Y.) 285
 Goldstein (J.A.) 72; 73; 77
 Goodblatt (D.) 69; 362; 441; 585
 Gooding (D.) 471
 Goodman (M.) 285
 Goranson (S.) 441
 Gordis (R.) 56; 86
 Gordley (M.E.) 285
 Gordon (R.P.) 96; 99; 100; 102; 124
 Gordón Gordón (F.) 124
 Goren (S.) 81
 Gorski (A.) 124
 Goshen Gottstein (M.) 84; 285; 460s; 464
 Grabbe (L.L.) 124; 441; 585
 Grappe (Ch.) 125
 Green (W.S.) 86; 87; 233
 Greenblatt (Ch.) 125
 Greenfield (J.) 69; 388; 543ss
 Greenspoon (L.) 285
 Gregory (B.C.) 585
 Grelot (P.) 399; 544
 Grierson (F.) 585
 Grossman (M.) 379; 441; 585
 Gruen (E.S.) 125
 Gruhn (V.I.) 418
 Gunneweg (J.) 125
 Günther (L.-M.) 285
 Gurewicz (S.B.) 460
 Gurtner (D.M.) 125; 441; 585
 Gusella (L.) 125
 Gzella (H.) 286
 Haaland (G.) 286
 Haberman (A.M.) 91
 Hacham (N.) 585
 Hadas-Lebel (M.) 239; 585
 Hahn (O.) 538; 441
 Hain (M.) 125
 Hakham (A.) 506; 520
 Hallermayer (M.) 286; 585
 Halpern (B.) 384
 Halpern-Amaru (B.) 161
 Halpern-Amaru (B.) 441
 Hamidović (D.) 586; 126; 511
 Hannah (D.) 126; 586
 Hanneken (T.) 286
 Hansen (D.U.) 441
 Haran (M.) 358; 441; 461; 466; 469
 Harkins (A.K.) 441; 586
 Harman (C.) 421
 Harrelson (W.) 126
 Harrington (D.J.) 126; 311
 Harrington (H.) 268; 356; 377; 483; 586
 Harter-Lailheugue (S.) 268; 475ss; 563s
 Hartman (L.F.) 73; 74
 Hauspsie (K.) 51
 Hawley (R.) 286
 Hayward (C.T.R.) 269
 Hayward (C.T.R.) 126; 286
 Healey (J.F.) 126
 Heath (J.) 286
 Hecke (P. Van) 586
 Heger (P.) **167-206**; 176; 177; 178; 189; 201; 286; 586
 Heichelheim (F.M.) 426
 Heim (C.) 420
 Heliso (D.) 586
 Hellerman (J.H.) 126
 Helyer (L.R.) 126
 Hempel (Ch.) 69; 127; 183; 249; 286; 384; 586
 Hengel (M.) 323; 328
 Henshke (D.) 127; 357; 358; 359; 360; 362; 364; 368
 Henten (J.W. van) 286; 586
 Henze (M.) 127; 286; 587
 Herbert (E.D.) 85; 93; 94; 94; 95; 96; 97; 98; 99; 100; 101; 102; 104; 127; 165; 463; 464
 Herms (R.) 441
 Herrmann (R.) 441
 Herzer (J.) 442

- Heshel (H.) 85; 529-531
 Heszer (C.) 348; 354;
 Hiebert (R.J.V.) 286; 466; 467
 Hieke (Th.) 286
 Hilhorst (A.) 384; 442; 567; 599;
 600
 Hillel (V.) 287; 462
 Himbaza (I.) 287; 587
 Hirschfeld (Y.) 287; 442; 480; 565
 Hjelm (I.) 127
 Hoekweld-Meijer (G.) 352
 Höffken (P.) 442
 Hoffman (Y.) 84
 Hoffmann (Ch.A.) **429-430**
 Hoftijzer (J.) 20; 39
 Hogan (K.M.) 442
 Høgenhaven (Y.) **247-259**; 287;
 442; 587
 Hollander (H.W.) 70; 546
 Holloway (P.A.) 587
 Holst (S.) 587
 Holtz (G.) 587
 Honigman (S.) 127
 Hoop (R. de) 127
 Hoppe (L.) 287
 Horbury (W.) 127
 Horsley (R.) 127; 287; 587
 Horst (P.W. van der) 127; 442; 587s
 Houston (W.J.) 128
 Houtman (A.) 362
 Huber (P.J.) 31; 39
 Hughes (J.A.) 342
 Hugo (Ph.) **93-108**; 588
 Huizenga (L.A.) 128
 Hultgren (S.) 287; **313-340**
 Humbert (J.-B.) 128; 480; 485
 Hunger (H.) 3; 23; 29; 39
 Hunt (A.) 588
 Huntington (E.) 417s
 Hurvitz (A.) 191; 469
 Huusola (H.) 84

 Ibba (G.) 128; 588
 Ilan (T.) 162
 Inowlocki (S.) 128; 287
 Isaac (E.) 128
 Isaksson (B.) 588
 Issar (A.) 419; 421; 423

 Jacobson (H.) 287
 Jaffé (D.) 287

 Janse (S.) 588
 Japhet (S.) 237; 442
 Jassen (A.) 588; 287
 Jastrow (M.) 545; 82; 218
 Jeffers (A.) 128
 Jensen (A.I.L.) 128
 Jensen (M.H.) 288
 Jiménez Bedman (F.) 128; 288
 Johns (L.L.) 129
 Johnson (D.) 129
 Johnson (S.R.) 129
 Johnston (Ph.S.) 129
 Jokiranta (J.) 155; 165; 189; 288;
 384; 442; 588
 Jol (H.) 442
 Jonge (M. De) 288
 Jongeling (K.) 20; 39
 Jonquière (T.M.) 288
 Joosten (J.) 163; 588; 443
 Joüon (P.) 234
 Jucci (E.) 288
 Juel (D.H.) 129

 Kaczynki (R.) 317
 Kaddari (M.Z.) 520; 509
 Kahana (A.) 326
 Kaiser (O.) 288
 Kalimi (I.) 129
 Kalman (J.) 288
 Kampen (J.) 178; 254; 288; 323;
 324; 325; 326; 327; 328; 332
 Kanael (B.) 68
 Kannigießer (B.) 538
 Kapera (Z.J.) 129; 443; 544; 588
 Kapfer (H.) 443
 Karrer (M.) 588
 Kasher (A.) 288; 289
 Katz (S.T.) 289
 Kaufman (S.A.) 20; 39
 Kazen (Th.) 129
 Kee (H.C.) 289
 Kehana (M.) 82
 Kennedy (C.J.) 129
 Kennicott (B.) 411
 Kerem (Z.) 129
 Kiel (M.) 443; 588
 Kim (D.H.) 85; 164;
 Kim (J.-H.) 289
 Kiraz (G.) 443
 Kister (M.) 130; 266; 289; 386;
 518

- Kittel (R.) 407
 Klawans (J.) 130; 589
 Kłęczar (A.) 289
 Klein (H.) 418
 Klinzing (G.) 242
 Klostergaard Pedersen (A.) 390
 Klostermann (A.) 102
 Klutz (T.) 130; 589
 Knibb (M.) 289; 589
 Knohl (I.) 492ss; 589
 Knop (A.P.) 130
 Koch (K.) 443; 589
 Koch-Westenholz (U.) 22; 39
 Köckert (M.) 130
 Koehler (L.) 47; 71; 234; 396
 Kohler (K.) 318; 321; 331; 332
 Konradt (M.) 289; 443
 Kooij (A. van der) 130; 290
 Koskenniemi (E.) 589; 290
 Kottsieper (I.) **93-108**; 130
 Kovelman (A.) 589
 Kraft (R.) 44; 130; 290; 353; 471
 Kratz (R.) 52; 130; 443
 Krauter (S.) 443s
 Krispenz (J.) 130
 Kugel (J.L.) 290; 346; 444; 558
 Kugler (R.A.) 130; 131; 194; 383; 388s; 444; 462; 550; 555; 559ss; 589;
 Kuhn (H.-W.) 131; 290
 Kuhn (K.A.) 444
 Kuhn (K.G.) 80
 Kukla (G.) 423
 Kutscher (E.Y.) 84; 493ss; 502; 505; 516
 Kvalbein (H.) 339
 Kvanvig (H.) 589

 Labahn (A.) 131
 Labahn (M.) 444
 Lanfranchi (P.) 444
 Lange (A.) 131; 168; 183; 184; 185; 249; 252; 290; 356; 384; 444; 589
 Langer (G.) 290
 Langer (R.) 356
 Langerman (T.) 131
 Langlois (M.) 131
 Laperrousaz (E.-M.) 131
 Lapin (H.) 354
 Latorre I Castillo (J.) 131

 Latour (É.) 444
 Lauterbach (J.Z.) 82
 Law (T.) 589
 Lawrence (J.D.) 131; 240
 Leicht (R.) 590
 Lemaire (A.) 132; 290; 463; 590
 Lemmelijn (B.) 459; 467; 472
 Leonhardt-Balzer (J.) 444
 Leoni (T.) 290
 Leslau (W.) 509; 520
 Lesses (R.) 290
 Leuenberger (M.) 290
 Levene (D.) 291
 Lévi (I.) 314; 316; 331; 332; 335
 Levine (B.A.) 70; 243; 362; 471
 Levine (L.I.) 71; 132; 191
 Levison (J.R.) 132
 Lewin (A.) 291
 Liber (M.) 316; 318; 321; 333; 335
 Libman (E.) 132
 Licht (J.) 233; 237; 240; 245; 254
 Lichtenberger (H.) 132; 233; 291
 Lidonnici (L.) 291
 Lieber (A.) 132; 291;
 Liebman (L.) 261
 Liebreich (L.J.) 318
 Liesen (J.) 132
 Lieu (J.) 132; 444
 Ligas (A.) 345
 Lim (T.H.) 291; 462; 526
 Lindars (B.) 472
 Linder (A.) 444
 Ling (T.) 444
 Liver (J.) 233
 Livneh (A.) 444
 Loader (W.R.G.) 291
 Loeb (I.) 316; 335
 Löhr (H.) 132
 Lönnqvist (K.) 132; 133; 291; 291; 444
 Loretz (O.) 211
 Lourie (B.) 133
 Luckenbill (D.D.) 397
 Luger (Y.) 314
 Lukaszewski (A.L.) 133
 Luker (L.M.) 133
 Lust (J.) 51; 471
 Luttikhuizen (G.P.) 355

 Maass (F.) 465
 Macadam (H.J.) 133

- Macaskill (G.) 292; 445; 590
 Macdonald (N.) 133
 Mach (M.) 590
 Machiela (D.) **395-403** ; 590
 Magaritz (M.) 421
 Magen (Y.) 445; 590
 Maggen (M.) 133
 Magne (J.) 465s; 472
 Magness (J.) 268; 292; 445; 479; 480ss; 565
 Maier (J.) 133; 169; 170; 172; 173; 174; 174; 175; 176; 316; 317; 318; 352; 365; 372; 378
 Main (E.) 590
 Makiello (Ph.) 445
 Malamat (A.) 397
 Malzer (B.) 538
 Mandel (P.) 292
 Mann (J.) 91; 314; 316; 318; 319
 Mantouvalou (I.) 538
 Manzi (F.) 292
 Maori (Y.) 445
 Marböck (J.) 208
 Marciak (M.) 292
 Marshak (A.K.) 590
 Martin (J.D.) 71
 Martin (M.) 84
 Martone (C.) 133; 292; 445; 590s
 Martyn (J.L.) 340
 Marx (A.) 133
 Mason (E.) 591
 Mason (S.) 292
 Mason (S.) 445
 May (H.G.) 254; 400
 Mazar (M.) 397
 McCarter (P.K.) 93; 99; 100; 103
 McConville (J.G.) 396
 McDaniel (K.J.) 292
 McDowell (M.) 134
 McGinnis (C.) 134
 McKeating (H.) 134
 McKechnie (P.) 134
 McLaren (J.S.) 293
 McLay (R.T.) 293
 McLean (N.) 405
 Mędala (S.) 134
 Méléze-Modrzejewski (J.) 445
 Melloni (G.Ph.) 446
 Meshorer (Y.) 68
 Metso (S.) 250; 591; 134; 252
 Metzenthin (Ch.) 446
 Metzger (M.) 558
 Meyer (R.) 465
 Mézange (Ch.) 134
 Michel-Nozières (C.) 39
 Mies (F.) 134
 Milgrom (J.) 158; 243; 362; 367; 368
 Milik (J.T.) 3; 4; 7; 11; 12ss; 39; 80; 83; 88; 91; 160; 310; 328; 372; 388; 529; 544ss
 Milikowsky (Ch.) 446
 Millar (F.G.B.) 591
 Millard (A.) 134
 Miller (C.L.) 229
 Miller (D.) 134
 Miller (G.D.) 591
 Miller (P.D.) 212
 Miller (S.S.) 354
 Mimouni (S.) 134
 Mirsky (A.) 353
 Mitchell (D.C.) 134
 Mittmann-Richert (U.) 293
 Miura (Y.) 591
 Mizrahi (N.) 446
 Moberly (W.) 293
 Montgomery (J.A.) 73
 Moo (J.) 446
 Moore (R.D.) 378s
 Moore (S.D.) 466
 Mor (M.) 293
 Morgan (T.) 135
 Morgenstern (J.) 329s
 Morgenstern (M.) 80; 446
 Morrow (W.S.) 591
 Mosca (P.G.) 338
 Muchowski (P.) 293
 Muddiman (J.) 446
 Müller (M.) 135
 Mumchugulu (K.) 135; 564
 Munnich (O.) 293
 Münz-Manor (O.) 350
 Muraoka (T.) 51; 163; 208; 209
 Murphy (F.J.) 135
 Murphy O'Connor (J.) 249; 250
 Nahe (Sh.) 493
 Najman (H.) 135; 193; 203
 Nakman (D.) 83; 88; 90; 91
 Nam (R.S.) **153-165**; 592
 Navarro Puerto (M.) 293
 Neev (D.) 419

- Negelauk (J.) 420
 Neher (M.) 293
 Nerga Menduïña (X..U.) 135
 Neubauer (O.) 6; 8; 12ss;; 39;
 356; 446; 471; 592;
 Newman (H.I.) 293
 Newman (J.H.) 446
 Newsom (C.) 43; 310; 384; 47;
 53; 54; 57; 59; 84; 592; 45
 Niccum (C.) 135
 Nickelsburg (G.) 32; 34; 40; 135;
 269; 293; 323; 336; 446; 592
 Nicklas (T.) 294; 446
 Niebuhr (K.-W.) 335
 Niebuhr (K.-W.) 336s; 447
 Niehoff (M.) 135
 Niemi (T.) 420
 Nihan (Ch.) 294
 Nikolsky (R.) 135
 Nir (R.) 592
 Nitzan (B.) 65; 187; 347; 592
 Noam (V.) 136; 294; 592
 Nodet (É.) 294; 592
 Noffke (E.) 294
 Noort (E.) 159
 Norin (S.) 397
 Noth (M.) 71; 418
 Nötscher (F.) 233
 Novick (Tz.) 350; 592
 Nus (A.) 420

 O'Brien (J.A.) 136
 O'Connell (S.) 136
 O'Dell (J.) 331
 Oakes (P.) 136
 Odasso (G.) 447
 Oegema (G.) 136; 294; 447; 593;
 Olson (D.C.) 593
 Olson (D.T.) 136
 Oppenheimer (A.) 64; 69
 Orlov (A.A.) 593; 295; 447
 Osten Sacken (P. von der) 249
 Ostmeyer (K.-H.) 447
 Oswald (J.) 64
 Oudshoorn (C.) 136; 295
 Owens (J.E.) 593

 Pardee (D.) 211; 212
 Parker (B.F.) 295
 Parker (V.L.) 447

 Parry (D.); 45; 93; 99; 100; 103;
 104; 136; 257; 447; 462; 593
 Parsons (M.C.) 593
 Passaro (A.) 560
 Pastor (J.) 295
 Patmore (H.M.) 593
 Patrich (J.) 447
 Paul (A.) 295
 Paul (S.M.) 44; 88
 Payne Smith (R.) 13; 15; 40; 545
 Pearce (S.) 448; 593
 Peels (E.) 342
 Perdue (L.G.) 593
 Pérez Fernández (M.) 137
 Perroni (M.) 448
 Peters (D.M.) 448
 Peters (N.) 208
 Petersen (A.K.) 593; 295
 Petersen (D.L.) 211
 Petraglio (R.) 137
 Petropoulou (M.-Z.) 593
 Peursen (W.Th. van) 295; 448;
 593s
 Pfann (S.) 137; 257
 Philonenko (M.) 137
 Pierce (Ch.T.) 295
 Pierlot (C.) 309
 Pietersma (A.) 296
 Pietsch (M.) 296
 Pilhofer (P.) 356
 Pines (S.) 202
 Pinnick (A.) 69; 344; 362
 Piovanelli (P.L.) 191; 448; 594
 Pisano (S.) 407
 Pistone (R.) 296
 Pitkänen (P.) 137
 Piwowarczyk (P.) 594
 Plicht (J. Van Der) 296
 Poirier (J.C.) 137
 Polacheck (I.) 137
 Pollak (F.) 84
 Polliack (M.) 296
 Polzin (R.) 237
 Pomykala (K.) 137
 Ponizy (B.) 296
 Poorthuis (M.) 358; 362
 Popkes (E.) 137
 Popović (M.) 137; 296; 342; 353;
 448; 594
 Porat (R.) 85; 296

- Porter (S.E.) 194; 377
 Porton (G.) 138
 Pratscher (W.) 594
 Preminger (A.) 225
 Preuss (J.) 565
 Price (J.I.) 448
 Propp (W.H.) 384
 Puech (E.) 43; 44; 61; 69; 138; 296; 297; 313; 314; 334; 335; 336; 338; 373; 374; 376; 384; 401; 430; 448; 462; 527; **543-561**; 567; 594; **569-572** ; **572-574** ; 594
 Qimron (E.) 44; 84; 138; 297; 364; 311; 336; 361; 363; 374; 381; 493; 498; 501; 504; 520; 520
 Quast (U.) 462
 Quincoces Loren (A.) 297
 Rabin (Ch.) 69; 244s;
 Rabin (I.) 138
 Rabinowitz (I.) 208; 209; 210; 217; 218; 219; 220; 221; 222; 465
 Rad (G. von) 395
 Rajak (T.) 449; 594
 Rand (M.) 354
 Rappaport (U.) 64; 297; 449
 Rasmussen (K.L.) 138
 Ravasco (A.) **405-413**
 Rebiger (B.) 449
 Redditt (P.L.) 139
 Reed (A.Y.) 297; 449
 Reed (S.A.) 530
 Reeder (Ph.) 449
 Regev (E.) 139
 Regev (E.) 297; 449
 Reich (R.) 449
 Reif (S.) 297; 346; 354; 356; 594
 Reimer (D.) 139
 Reiner (E.) 22; 40
 Reinhartz (A.) 297
 Reiterer (F.V.) 139; 297; 594s
 Rey (J.-S.) **269-270**; 298; 449; 595
 Reymond (E.) 139; **207-231** ; 595
 Reynolds (B.H.) 449
 Reynolds (S.) 416
 Riaud (J.) 139; 239
 Richards (K.H.) 211
 Ricks (S.S.) 462
 Riedo-Emmenegger (Ch.) 298
 Rietz (H.) 139; 298
 Rin (S.) 505; 520;
 Riska (M.) 595
 Roberts (J.J.M.) 139
 Rocca (S.) 298
 Rochberg -Halton (F.) 34s; 40
 Rodgers (Z.) 298
 Rodríguez Torné (I.) 139
 Rofé (A.) 88; 139; 412; 449; 471; 595
 Roig Lanzillotta (L.) 595
 Roitman (A.) 139; 140
 Ron (Z.) 419
 Rosen-Zvi (I.) 140
 Rossetti (M.) 450
 Roth (C.) 68
 Rothschild (C.K.) 298
 Rothstein (D.) 81; 83; 298; 360; 361; 450
 Rouillard (H.) 67
 Rowley (H.) 66
 Ruffatto (K.J.) 140; 596
 Ruiten (J.T.A.G.M. van) 140; 454; 578; 596; 298
 Runesson (A.) 596
 Ruzer (S.) 298; 450
 Ryzhik (M.) 491; 513
 Sacchi (P.) 140; 596
 Sachau (E.) 560
 Sachs (A.) 29; 40
 Saddington (D.) 450
 Sæbø (M.) 170
 Safrai (B.) 298
 Safrai (S.) 321
 Safrai (Sh.) 357
 Safrai (Sh.) 395; 398
 Safrai (Z.) 358; 395; 398
 Sahm (U.W.) 298
 Saley (R.J.) 93
 Salvadó (N.) 140
 Salvesen (A.) 140
 Sánchez Caro (J.M.) 140
 Sanders (J.A.) 208
 Sanders (J.T.) 450
 Sanders (P.) 369

- Sanders (S.L.) 299
 Sanders (J.A.) 209; 210; 211; 215; 217; 218; 219; 222; 298; 460; 464s; 466; 468; 470
 Sanderson (J.) 525
 Sandoval (T.) 450
 Snger (D.) 140
 Snger (D.) 299
 Sanmartn (J.) 20; 39
 Sarason (R.) 356
 Sauer (G.) 140
 Saulnier (S.) 450
 Scatolini Apstolo (S.S.) 299
 Schfer (P.) 68; 177; 299; 316; 318; 333
 Schattner-Rieser (U.) 314; 317; 319; 320; 544; 558
 Schenker (A.) 299
 Scherman (N.) 314; 315
 Schicklberger (F.) 410
 Schiffman (L.H.) 44; 84; 140; 141; 157; 168; 169; 170; 172; 186ss; 201; 205; 208; 243; 299; 335; 336; 361; 363; 364; 366; 369; 430; 450; 461; 463; 596
 Schimanowski (G.) 450
 Schipper (B.U.) 596
 Schipper (J.) 450
 Schlieer (B.) 299
 Schmelzer (M.) 354
 Schmid (K.) 299
 Schmidt (F.) 596; 141; 450
 Schmitz (B.) 141
 Schniedewind (W.M.) 153; 163
 Schofield (A.) 597; 299
 Schkel (L.M.A.) 336
 Schorch (S.) 597; 299
 Schrage (W.) 451
 Schreckenber (H.) 451
 Schreiber (S.) 597
 Schreiner (J.) 316
 Schremer (A.) 141
 Schuller (E.) 141; 162; 194; 310; 320; 338; 342; 343; 348; 462
 Schulthess (F.) 40
 Schultz (B.) **63-77** ; 141
 Schpphaus (J.) 331
 Schwartz (D.R.) 69; 142; 300; 358; 362; 451; 597
 Schwartz (J.) 300; 362; 400; 566; 597
 Schwartz (S.L.) 300; 451
 Schwienhorst-Schnberger (L.) 597
 Scott (R.B.Y.) 416; 419
 Segal (A.F.) 300
 Segal (M.) 69; 80; 300; 597; 451; 467
 Segal (M.Z.) 89; 223; 353; 521; 511;
 Sekki (A.E.) 254
 Seland (T.) 597
 Sen (F.) 142; 300; 451; 597
 Shadeh (N.) 418
 Shalim (N.) 418
 Shamir (O.) 142
 Shanks (H.) 300; 426; 567; 598
 Sharon (M.) **109-149** ; 508; 521
 Sharon (N.) 300; **271-306** ; **431-456** ; 507
 Sharvit (S.) 511; 521
 Shatzman (I.) 69; 300
 Shemesh (A.) 187; 143; 268; 360; 361; 451s; 598
 Shepherd (D.) 598
 Shor (P.) 261
 Siegert (F.) 143; 301; 452
 Sievers (J.) 143
 Sigal (Ph.) 598
 Simon (U.) 346
 Sinclair (L.A.) **415-427**
 Singer (I.) 65
 Singer (S.) 314
 Sjberg (M.) 452
 Skehan (P.W.) 75; 208; 209; 215; 216; 219; 220; 221; 222; 223; 224; 225; 229; 461; 464; 466
 Smalley (W.A.) 211
 Smend (R.) 208
 Smiles (V.) 143
 Smith (B.D.) 452
 Smith (G.A.) 417
 Smith (M.) 466
 Smith (M.S.) 217
 Smith (S.) 23
 Smith (S.) 40; 467; 468s
 Snaith (J.G.) 226
 Sokoloff (M.) 15; 41; 237; 547; 552; 545
 Soll (W.M.) 143
 Song (Ch.H.) 301
 Spencer (P.E.) 598
 Spigelman (M.) 143

- Spottorno y Díaz-Caro (M.V.) 301
 Stacey (D.) 452
 Staley (J.) 598
 Stec (D.M.) 143
 Stegemann (H.) 57; 59; 60; 159;
 198; 247; 336s;
 Steiner (R.C.) 397; 399
 Stemberger (G.) 143; 352
 Stendahl (K.) 144
 Stenschke (Ch.) 598
 Stern (E.) 301
 Stern (M.) 64; 86
 Steudel (A.) 60; **93-108**; 144; 301;
 452; 598
 Steussy (M.J.) 144
 Stichel (R.) 301
 Stökl (J.) 301
 Stökl Ben Ezra (D.) 452; **533-542**
 Stone (M.E.) 44; 69; 70; 158;
 257; 331; 343; 388; 301; 302;
 452; 543ss; 598
 Strawn (B.A.) 144
 Strawn (B.A.) 452
 Strazicich (J.) 598
 Strugnell (J.) 47; 159; 265; 309-
 311; 328; 365; 472
 Stuckenbruck (L.T.) 69; 269; 302;
 598
 Sullivan (K.) 302; 598
 Sulzbach (C.) 144; 302; 598
 Sussmann (Y.) 187
 Suter (D.) 599; 144
 Swanson (D.) 144; 165
 Swartz (M.D.) 350; 353

 Tabor (J.) 268; 335; 475ss
 Tabory (J.) 321; 356
 Tackeray (H.St J.) 405
 Tak (J.G. Van Der) 302
 Tal (A.) 15; 20; 41
 Talmon (Sh.) 44; 84; 86; 144;
 302; 452; 460; 463; 464; 471;
 533ss; 539; 541; 599;
 Talshir (D.) 491; 599
 Talshir (Z.) 599; 452
 Tantlevskii (I.) 144
 Tcherikover (V.) 358
 Tenney (F.) 426
 Termini (C.) 302; 453
 Teugels (L.) 453

 Thomas (S.) 453
 Thompson (Th.L.) 144
 Tigay (J.H.) 91; 397
 Tigchelaar (E.J.C.) 4; 7; 17; 39;
371-381; 145; 241; 248; 251;
 252; 254; 302; 303; 342; 384;
 453; 462; **525-527**; 529s; 567;
 599; 600
 Tilly (M.) 303
 Tollington (J.E.) 303
 Tomasino (A.J.) 453
 Tomson (P.J.) 599
 Tov (E.) 44; 45; 46; 59; 61; 83;
 84; 85; 86; 87; 90; 91; 92; 145;
 153; 155; 155; 156; 157; 159;
 162; 163; 164; 165; 261; 303;
 310; 373; 385; 407; 429; 453;
 461; 463; 464; 467; 468; 471;
 471; 472; 473; 496; 521; 530;
 538; 599s
 Trafton (J.L.) 145
 Trapp (Th.H.) 64
 Treballe Barrera (J.) 146; 154;
 198; 303; 429; 453; 461; 471;
 600
 Triebel (L.) 303
 Troiani (L.) 453; 600
 Tromp (J.) 146; 453; 600
 Troxel (R.L.) 229
 Tuinstra (E.W.) 383
 Turner (D.L.) 146
 Tur-Sinai (N.H.) 509
 Turton (A.) 146
 Tuschling (R.M.M.) 303
 Tuzlak (A.) 146

 Ulrich (E.) 75; 85; 93; 94; 94;
 95; 96; 103; 104; 164; 165; 238;
 257; 304; 336; 401; 407; 412;
 454; 460; 461; 462; 463; 467;
 470; 471; 472

 Vajda (G.) 513
 Van Seters (J.) 397
 Vandenabeele (P.) 146
 Vanderkam (J.) 32ss; 41; 68; 71;
 72; 146; 157; 160; 161; 191;
 208; 304; 336; 347; 366; 430;
 454; 462; 463; 527; 600
 Vanlandingham (Ch.) 454

- Vargas (I.) 147
 Vázquez Allegue (J.) 147; 304
 Vegas Montaner (L.) 154; 198; 429; 461; 601
 Venter (P.M.) 601
 Venturini (S.) 407; 410; 413
 Verderame (L.) 22; 41
 Verheyden (J.) 601
 Vermes (G.) 91; 195; 250; 304; 340; 378
 Vialle (C.) 601
 Vielhauer (R.) 147
 Vignolo (R.) 304
 Virolleaud (Ch.) 22; 41
 Vladimirov (V.) 147
 Vogel (M.) 454
 Volgger (D.) 147
 Volgger (D.) 304

 Waard (Jan de.) 304
 Wacholder (B.Z.) 72; 82; 159; 194; 197; 304; 461
 Walck (L.W.) 601
 Wallis (L.) 400
 Wassen (C.) 147; 454; 601
 Water (R. van de) 146
 Waters (G.) 601
 Watson (F.) 304
 Watts (R.) 454
 Wechsler (M.G.) 147
 Weidner (E.F.) 22; 41
 Weigl (M.) 454
 Weigold (M.) 304; 147
 Weinberg (J.P.) 237; 238
 Weinfeld (M.) 147
 Weis (H.) 423
 Weiss (Z.) 304; 601
 Weissenberg (H. Von) 305
 Weissenberger (M.) 454
 Wenell (K.J.) 454
 Wenthe (D.O.) 147
 Werline (R.A.) 601
 Werman (C.) 147; 148; 305; 454
 Wernberg- Møller (P.) 254
 Werret (I.) **261-268**; 305; 455; **475-489**; 563ss; 601
 West (J.) 148
 Westerholm (S.) 305
 Wevers (J.W.) 398
 Wevers (J.W.) 305

 White Crawford (S.) 153; 154; 156; 157; 159; 161; 278; 279; 390ss; **429-430**; 436; 526; 580
 Whitney (K.W.) 455
 Whyte (R.) 420
 Wildberger (H.) 64; 75
 Wilk (F.) 455
 Williams (C.H.) 148
 Williams (M.H.) 455
 Wilson (J.R.) 455
 Winslow (K.S.) 601
 Winston (D.) 305
 Winter (S.) 148
 Wise (M.O.) 159; 173; 335; 363; 365; 366; 377; 378; 429
 Wiseman (D.J.) 397
 Witmer (S.E.) 305
 Wold (B.G.) 305; 148
 Wolff (T.) 538
 Wong (K.L.) 305
 Wood (B.) 419
 Wooden (R.G.) 455
 Wouters (H.) 148
 Wright (A.T.) 269s; 305
 Wright (B.G.) 148; 305; 455; 601
 Wright (G.E.) 418; 526
 Wright (N.T.) 148
 Wright (R.B.) 331; 325; 455
 Writh (E.) 424

 Xeravits (G.) 67; 68; 148; 306; 602

 Yadin (Y.) 65; 80; 84; 88; 159; 198; 244; 359; 360; 361; 362; 365; 368; 403; 534; 540s
 Yahalom (J.) 346; 349; 353; 355
 Yakin (D.) 421; 504
 Yardeni (A.) 148; 491ss; 602
 Yaron (H.) 521
 Yechiele (Y.) 422
 Yeivin (I.) 500; 510; 512; 521
 Yishai (R.) 148; 455
 Yuditsky (A. [E.]) 602

 Zahn (M.) 165
 Zak (I.) 421
 Zangenberg (J.) 455; 602
 Zappella (M.) 148
 Zenger (E.) 460; 463; 466

Zerbe (G.M.)	149	Zimmermann (J.)	314; 335; 336s
Zevit (Z.)	237	Zlotowitz (M.)	314; 315
Zewi (T.)	456	Zollshan (L.T.)	306
Zias (J.)	268; 306; 456; 475s; 477; 563-567	Zsengellér (J.)	306
Ziegler (J.)	213; 510	Zuckerman (K.)	403

RÉFÉRENCES BIBLIQUES

ANCIENT TESTAMENT

<i>Genèse</i>		34,15	545
1,29	49	34,16	547
6,1-4	269s	34,17	547
7,9	195	34,20	550
9,5	183	34,25	550; 553
10,4	65	34,25-26	549
10,5	65	34,26	550
12,6-8	400	34,30	549; 552; 553
12,8-9	401	35,7	170
13,15-16	401	37,17	215
19,2	223	37,18	216
19,25	506	37,21-22	550
20,7	500	37,29-30	550
20,13	215	39,14	541
21,12	402	39,17	541
21,14	215	40	184
22	389	41,39	541
25,20	161; 399	42,21	181
25,25	355	42,22	550
26,10	181	42,37	550
27,11	355	45,17	102
27,45	160	47,21	50
28,5	399	48,16	503
30,18	224	49,3	513
31,20	399	49,7	513
31,24	399	49,10	184
31,49	170		
33,19	550	<i>Exode</i>	
34,2-3	545	1,13	55
34,5	550	1,19	216
34,5-6	551	3,6	170
34,7	545; 550	10,23	507
34,8	221; 547	12,20	508
34,13	545s; 547	12,25	192

12,40	507
13,1-10	82
13,10	203
13,11-16	82
14	389
15,3	320
17,5	155
18,5	222
18,21.25	56
18,23	216
20,5	497
20,18	387
21,16	181
23,16	196
23,19	177
24,9	156
25,2	234; 238
26,22-28	170
27,6	65
27,20	513
32,20	180
34,5	396
34,22	196
35,21	234; 238
35,29	234; 238
38,8	407
<i>Lévitique</i>	
4,16	155
7,7	175
7,16	234
7,34	361
10,16	174
15,14	367
15,32	47
16,12	180
17,34	361
18,6	170
19,23	200
21,16-24	243
21,17-23	244
22,21	233; 242; 243
23,4	193
23,10-14	200
23,15-17	200
23,17	508
23,43	430
24	158
24,1	430
24,2	513
25,2-7	193

25,3-13	200
25,8-13	193
26,40	51
27,34	189; 202
<i>Nombres</i>	
4,23	407
5,2	367
5,13	170
12,2	223
15,20	200
15,20-21	265
15,37-41	318ss
17,8	195
18,12	158
18,21.24	200
20,1	507
20,16	55
22,26	513
24	67-77
24,15-18	387
24,24	63; 64; 65; 74s; 76;
77	
25,11-13	234
27,20	220
29,39	234
32,10	50
33,8	156
33,49	199
<i>Deutéronome</i>	
1,15	58
1,21	57
4,20	52; 54; 55
5,6-18	83
5,9	497
5,15	55
6,4-9	82; 180
6,7	223
6,21	55
7,3	47; 48
7,6	52
7,22	506
10,11	58
11,12	174
11,13-21	82
12,5	359
12,5-7	361
12,9	199
12,21	361; 364
12,26	364

13,1	189; 201	<i>Josué</i>	
13,15	172; 174	1,5-6	57
14,2	52	1,6	53
14,23	360	1,6-7.9.18	57
14,24-25	360	1,12-15	56
15,6	52	1,16-18	56; 59
16,15	360	1,17	54; 55
16,18	58	1, 17-18	58
17,4	172; 175	1,18	55; 56
17,7	175	5,9	170
17,8-13	182	5-6	57
17,9	172	6,26	59
17,10	174; 175	8,1	57
17,11	174	8,33	56
17,17	197	9	60
17,18	175	12-19	45
17,19	175	13-14	44
19,18	172	14,6	49
22,2	174	14,10	55
22,3	396	21	44
23	488	23,2	56
23,12-14	475	23,14	409
23,12-15	483	24,2-3	71
24,19	526		
24,22	525 ^{ss}	<i>Juges</i>	
26,5	395-403	5,2	234; 238
26,8	55	5,9	234; 238
26,18	52	6,29	172
28,30	53	7,11	57
28,15	51	21,16	155
28,43	52	9,2	551
28,45	51		
28,45-57	49	<i>Ruth</i>	
28,64	50; 51	3,14	215
28-31	58		
29,12-21.26	49	<i>I Samuel</i>	
29-31	51	1,3	104
30,1-6.8-9	52	2,1-10	315
30,1-9	52	2,5	333; 334
31,1-5	52	2,6	321; 333s
31,7-8	57	2,7	334
31,17	49; 170	2,7-9	333
31,21	48; 49	2,8	334
32	373	2,10	333; 339
32,4	56	2,15	216
32,17	49	2,20	105
32,21	221	2,21	104
33,1	49	2,22-23	405-408
33,8-11	387	3,14-17	405
34,8-9	59	3,19	408-409

4,4	104	6,4	505
4,11	104	6,17-20	200
5,8	410	8,56	409
5,9	409-412	11,13	357
6,2	105	11,32	357
6,5	220	11,36	357
6,8	195	12,32,33	20
6,11	411	14,21	357
6,21	105	14,22	221
7,15	196		
9,3	396	2 <i>Rois</i>	
9,9	174	5,20	399
9,15	170	6,32	216
9,20	396	11,4,9,11	56
14,8	170	13,17,19	51
16-17	459; 468-471	21,4	357
18,7	220	21,7	357
18,17-18	405	21,14	49
20,12	170	22,5	195
20,25	507	22,8	195
21,10	493	22,13	196
27,12	105	23,22	196
2 <i>Samuel</i>		1 <i>Chroniques</i>	
2,7	57; 94	1,7	65
3	93-108	3,1	101
4,1-2	94	9,17	234
5,1	105	14,16	217
5,8	104	16,8-36	317
6,20	170	17,21	495
7,5-6	507	19,25	220
10,3	104	20,8	506
12,31	104	22,13	57
13,8	265	22,19	223
13,28	105	28,11-18	190
13,32	493	28,20	57
13,37	104	29	233
15,19	170	29,5-6	234
19,10	105	29,6	237
19,12	195	29,9	236s
20,6-10	405	29,14	236s
21,16-18	405	29,17	236s
21,22	506	29,24	503
22,6-13	405		
23,7	405	2 <i>Chroniques</i>	
23,9-12	405	12,14	222
1 <i>Rois</i>		17,16	234; 238; 325
1,21	503	29,5	238
1,51	503	30,7	51
3,7-9	213	31,1	51
		31,14	234

34,21	196	7,12-13	326
35,18	196	7,12-14	328
36,14	47	7,13	322; 325; 327
		7,17	330
<i>Esdras</i>		7,18	326
1,6	233; 238	8,5	65; 66; 73
2,68-69	233; 238	42-54	77
3,5	234; 236; 238		
3,8	430	<i>2 Maccabées</i>	
5,9	497	1,19-36	195
6,22	65	2,1-5	195
7,6-9	156	4,9-16	358
7,10	172; 174	6,4-5	358
7,13	234	7,9	322
7,15-16	234	7,11	322
9,14	51	7,14	322
10,2	51	7,23	322
		14,6	322s
<i>Néhémie</i>			
1,8	51	<i>Isaïe</i>	
5,1-13	329	1,11-17	242
8,7	174	1,17	501
8,14	196	5,20	512
10,33-36	429	6,1	506
10,35	158	7,7	75
11,2	233; 234; 236; 238	8,8	75
13,27	51	10,23	75
		10,25	75
<i>Tobie</i>		14,19	101s
14,2	553	19,17	514
		23,1,12	65; 70; 72; 74
<i>Esther</i>		23,12-13	64; 75; 76
2,6	496	23,13	71; 77
9,21	20	28,4	266
		28,16	239; 240; 241
<i>1 Maccabées</i>		29,5	180
7,12-18	323	29,17	510
1,1	64; 65; 66; 73	29,24	203
2	325	30,20	513
2,29	324	33,21	71
2,42	238; 322; 324; 326;	42,9	320
328; 331		42,13	320
2,51-60	334	43,5	265
2,64	334	43,18-19	320
5,17	327	48,17	220
6,2	73	50,4	222
7	324	54,16	173
7,5	327	58,6-12	329
7,9-13	326	59,1-8	329
7,10-13	323	59,14-15	329
7,12	327; 330	63,1	320
		66,7	216

<i>Jérémie</i>		11,33	183; 325
1,6-7	213	11,36	75
2,10	70; 72; 74	11,40-45	63; 64; 69
4,18	512	12,2	323
7,18	265	12,3	322; 493
13,16	220		
23,3	265	<i>Osée</i>	
24,9	507	1,14	501
25	392	2,12	170
30,3	495	7,4	265; 268
31,8	265	14,15	234
31,10	265		
32,37	265	<i>Amos</i>	
33,3	495	4,6	507
40,4	55	5,9	513
		5,22-25	242
<i>Lamentations</i>		9,9	50
5,8	52		
		<i>Habacuc</i>	
<i>Ézéchiel</i>		1,6	189
2,37	49	3,3	220
3,12	506		
3,23	507	<i>Sophonie</i>	
8,12	217	3,19-20	265
16,6	500		
16,47	509; 516	<i>Aggée</i>	
20,34	265	2,6	509
20,41	265		
27,6	65; 70; 72; 75; 77	<i>Zacharie</i>	
27,10	74	7,3	156
27,31	512	8,9	156
30,9	71; 75	8,22	222
32,22	74	11	268
34,5.8	49	11,7-14	264; 265
34,8	50	14,5	338
34,16	396		
37,10	493	<i>Malachie</i>	
39,26	51	3,5	329
46,12	234	3,16-18	329
<i>Daniel</i>		<i>Job</i>	
8,13	338	1,8	56
9	392	2,3	56
9,7	51	4,11	396
9,16	503	5,1	338
9,27	75	15,10	497
11	66; 74; 77	15,15	338
11,7	75	29,13	396
11,10.40	75	31,19	396
11,21	220	33,8	513
11,30	65; 71; 73; 75; 77	36,27	218

<i>Psaumes</i>		122	61
6	531	145,10	329
6,11	320	145,14	315
8,2	220	145-150	316ss; 321
10,1	49	146,6	334
11,1	324	146,8	315
12,2	329	151 LXX	459-473
16,3	505	154	227
16,10	329		
18,26-28	329	<i>Proverbes</i>	
24,3	508	1,24	216
24,8	513	1-9	380s
25	224	2,17-19	374
29,2	220	2,19	376s; 379
30,5	329	4,4	500
31,24	329	5,9	220
31,25	334	7	210
32,6	329	7,20	20; 500
34	224	7,27	377
37,9	329	8,17	216
37,11	329	8,25	215
37,22	329	9,1-6	210
37,28	329	11,13	170
37,28-29	328	11,20	245
37,29	329	14,14	493
37,34	329	15,8	245
37,35	329	15,29	245
47,9	506	17,21	223
50,5	329	24,7	221
54,8	234	28,6	216
66,4	350	31,6	396
68,7	320; 321	31,7	398
77,15	320		
78,49	505	<i>Ecclésiaste (Qohelet)</i>	
79,2	327; 329	3,11	217
79,2-3	330	3,14	201
81,4	20		
89	339	<i>Cantique</i>	
89,6-8	338	5,2-4	228
89,37-38	338	7,3	493
90,2	215	8,6	228
97,10	329		
103,2	33	<i>Sagesse de Salomon</i>	
106,2	319; 321	7,7	216
106,8	320	8,1	210
106,30	234	8,2	217
106,39	47		
107,4	203	<i>Ecclésiastique (Ben Sira)</i>	
109,10	50	3,21-23	185
119,133	342	4,18	222
119,176	396	6,31	219

6,36	221	33,16	218
7,20	223	36,3-6	320
11,4	512	38,34	223
14,21	222	39,1-2	185
14,23	229	39,6	185
14,23-24	210	42,15-43,33	353
15,2	210; 229	42,18	222
15,2-10	219	43,9-18	216
21,22-23	229	44,1-15	353
24,21	219	45,23	234
30,21	223	51	333
32,9	221	51,13-30	207-231

NOUVEAU TESTAMENT

<i>Matthieu</i>		<i>Actes des Apôtres</i>	
2,2	68	11,25	426
11,2-6	314		
11,2-6	340	<i>Galates</i>	
21,1	505	3,19	337
26,36	505		
		<i>Hébreux</i>	
<i>Marc</i>		11,8-10	396
5,12	270		
14,32	505	<i>2 Pierre</i>	
		1,19	67
<i>Luc</i>			
7,18-23	314; 340	<i>Apocalypse</i>	
19,29	505	22,16	67

RÉFÉRENCES AUX TEXTES DE QUMRAN⁽¹⁾

1QIsa^a		1QpHab (<i>1Qpesher Habacuc</i>)	
<i>Ensemble</i>	383	<i>Ensemble</i>	383
1Q5 (<i>1QDeut^b</i>)		<i>Ensemble</i>	538
<i>Ensemble</i>	373	I,13	192
1Q11		II,10-11	189
V,3	236	II,12-13	66
1Q13		II,2.8.14	192
<i>Ensemble</i>	88ss	II,2-3	184
1Q14		II,5-8	192
8-10 6-7	183; 186	VI,13-VII,5	559
8-10 7	235; 236	VII,4	186
1Q20		VII,4-13	170
XX,15	545	VIII 17	571
XX,28	572	X,9	181
1Q21		XI,1	170
48	553	XII 15	571
1Q22		1Q20	
1 iii 1	50	(<i>1QapGen; Apocryphe de la Genèse</i>)	
12 3	571	V,18	552
I,6	202	XI,11	402
IV,4	202	XI,14	552
		XIV,12	402
		XIX,8	395-403
		XXI,12-13	401
		1Q29	
		13	251

(1) La classification des textes qumrâniens suit l'ordre établi dans la "List of the Manuscripts from Qumran" dans F. GARCÍA MARTÍNEZ *The Dead Sea Scrolls Translated The Qumran Texts in English. Second Edition*, Brill-Eerdmans; Leiden-Grand Rapids, 2004.

13 3,	571
13-17	248; 258
1QS (<i>Règle de la Communauté</i>)	
<i>Ensemble</i>	383; 538
I,11	235; 237
I,1-2	185
I,1-3	179; 181
I,7	235; 236
I,8	244
II,4-9	258
III,13-IV,26	183; 247-259
IV,6	182
V,1	236
V,1-3	250
V,2-3	239
V,5	240; 241
V,5-6	239
V,6	236; 241
V,7,20	183
V,9-11	170
V,10	236; 237
V,11	171; 181
V,11-12	173; 181; 185; 203;
204	
V,12	170
V,20-21	179
V,21-22	236
VI,6	173; 179
VI,6-7	179; 180
VI,8	240
VI,13-14	236
VII,7-8	241
VII,11-12	173
VIII,4-10	240
VIII,7-8	239; 240
VIII,9	241
VIII,10	241
VIII,11-12	171; 179; 180; 197
VIII,15	170; 173
VIII,16	170
IX,1	181; 182; 203
IX,3-4	244
IX,3-6	240; 242
IX,4	241
IX,4-5	240s
IX,4-6	475
IX,5	233; 243; 244
IX,6	241; 245
IX,9	241
IX,12	183

IX,12-19	252
IX,16-17	181
IX,17	191
IX,18	182
IX,24	234s; 245
XI,35	182
XIII,15	176

1Q28a*(1QSa 1QRègle de la Congrégation)*

I,28	245
I,14	56
II 11	571
II,4-7	244

1Q28b*(1Q Sb Recueil des Bénédictiones)*

I,1	183
III,22	183
III,22-23	182
IV 23	571
IV,23-25	337
V,20	183
V,27-28	67

1QM (*Règle de la Guerre*)

<i>Ensemble</i>	383; 538
I	69
I,2	63; 64
III,17	56
IV,2	56
V 10,	571
VII,4-5	244
VII,5	235; 237; 243
VII,6-7	475; 478ss
X,6	325
XI,6-7	67
XIV,7	244; 245
XV,7	57
XVIII,12	55

1QH^a (*1QHodayot 1QHymnes*)

<i>Ensemble</i>	383; 538
IV,9	171
V,1	252
V,14	215
VI,26	234; 235
VI,37	380
VII,13	234
VII,17	215
IX	190

IX,7	215	4Q57 (<i>4QIsa^c</i>)	
IX,10-11	215	9 ii 17-18	75
IX,16-20	258		
IX,19-20	215	4Q87 (<i>4QPs^e</i>)	
IX,21	170; 182	<i>Ensemble</i>	463s
IX,28	215		
IX,36	244; 245	4Q88 (<i>4QPs^f</i>)	
X,13	182	<i>Ensemble</i>	464
XII,9-11	181		
XII,27-28	182	4Q123	
XIV,4	170	<i>Ensemble</i>	44
XV 27	571		
XX,4	252	4Q128 (<i>4QPhyl^a</i>)	
XX,11	252	<i>Ensemble</i>	88ss
XXI,4-5	170		
XXIII 26	571	4Q129 (<i>4QPhyl^b</i>)	
XXV,10	252	<i>Ensemble</i>	88ss
XXVI,14-15	171		
1Q31		4Q130 (<i>4QPhyl^c</i>)	
1 1	235	<i>Ensemble</i>	88ss
2Q24		4Q131 (<i>4QPhyl^d</i>)	
4 13	497	<i>Ensemble</i>	79
3Q15		4Q132 (<i>4QPhyl^e</i>)	
<i>Ensemble</i>	538	<i>Ensemble</i>	79
4Q14 (<i>4QExod^e</i>)		4Q133 (<i>4QPhyl^f</i>)	
35	525	<i>Ensemble</i>	79
36	525	4Q135 (<i>4QPhyl^h</i>)	
38	525	<i>Ensemble</i>	88ss
4Q22 (<i>4QpaleoExod^m</i>)		4Q136 (<i>4QPhylⁱ</i>)	
XXVI 32	156	<i>Ensemble</i>	88ss
4Q29		4Q137 (<i>4QPhyl^j</i>)	
<i>Ensemble</i>	49	<i>Ensemble</i>	88ss
4Q30 (<i>4QDeut^c</i>)		4Q138 (<i>4QPhyl^k</i>)	
<i>Ensemble</i>	49	<i>Ensemble</i>	88ss
4Q34		4Q139 (<i>4QPhyl^l</i>)	
3 5-7	526	<i>Ensemble</i>	88ss
4Q51 (<i>4Qsam^a</i>)		4Q140 (<i>4QPhyl^m</i>)	
<i>Ensemble</i>	93-108	<i>Ensemble</i>	88ss
III	405-408		
IV	408-409	4Q141 (<i>4QPhylⁿ</i>)	
V	409-412	<i>Ensemble</i>	88ss

4Q142 (4QPhyl ^o)	1-2 i	571
<i>Ensemble</i> 88ss	2 12	192
	14	173
4Q143 (4QPhyl ^p)		
<i>Ensemble</i> 88ss	4Q175 (4QTest)	
	<i>Ensemble</i>	387
4Q144 (4QPhyl ^q)	12-13	67
<i>Ensemble</i> 88ss	21-30	45
4Q145 (4QPhyl ^r)		
<i>Ensemble</i> 88ss	4Q176	
	<i>Ensemble</i>	538
4Q158 (4QRP ^a)	16 3	215
<i>Ensemble</i> 391	4Q177 (4QCatena ^a)	
4Q161 (4QpIsa ^a)	1-4 10	571
8-10 3,11	5-6,8-9	324
8-10 17		
4Q162	4Q180	
II,1	1 1-4	258
4Q163	4Q184	
4-6 ii 12	1	371-381
	3 5	373
4Q164	4Q186	
1 7	1 i 9	571
4Q165	4Q196	
1-2 3	3 10	571
	18 15	553
4Q167 (4QpHos ^b)	4Q198	
<i>Ensemble</i> 267	1 1	553
4Q169 (4QpNah)	4Q201	
3-4 2	1	571
3-4 iii	1 i 3	571
	1 i 7	572
4Q171 (4QpPs ^a)	1 iv 8	571
1-10 ii 11	3,3	497
1-2 i 26	4Q203 (4QEn ^a)	
1-2 ii 10	7b i 3	571
3-10 iv 4		
III,15-16	4Q206 (4Qen ^d)	
4Q173	3 i 5	572
5 5	4 ii 6	571
4Q174 (4QFlorilegium)	4Q208 (4Qen ^e)	
1 2 i 6	<i>Ensemble</i>	3-41
1 i 21		

4Q209 (*4QEnastr^b*)
Ensemble 3-41
 23 4. 571

4Q210 (*4QEnastr^c*)
Ensemble 11ss

4Q211 (*4QEnastr^d*)
Ensemble 11ss
 1 i 3 571; 572

4Q212 (*4QEn^s*)
 1 iii 21-22 33

4Q213
 1 i 12 555
 1 i 13 555
 1 i 14 556
 1 i 15 556
 1 i 20 556
 1 i-ii 543

4Q213a
 3 548; 551; 555
 3 15 571
 3 16 571
 3 17 571
 3-4 560
 3-4 18 571

4Q213b
 1 1 559; 571
 6 560

4Q214a
 2 ii 543

4Q214b
 5-6 ii 560

4Q215a
 1 2.8 215
 1 ii 257-259
 1 ii 4 571

4Q216
Ensemble 538

4Q225
 2 ii 8-10 559

4Q228
 1 ii 4 264

4Q231a
 3 14 572

4Q243
 11 ii 1 571

4Q246
 1 i 2 571

4Q251
 1 5 184
 9 5 571

4Q252 (*4QBenPatr 4QpGen^d*)
Ensemble 392

4Q256 (*4QS^b*)
 IX,1-3 250
 IX,3 239

4Q257 (*4QS^c*)
 V 258
 V,4-6 251

4Q258 (*4QS^d*)
 I,1 183
 I,1-3 250
 I,2 239
 II,1-2 236
 VII,5 235
 VIII,1-2 181

4Q259 (*4QS^e*)
Ensemble 538
 II,10-11.18 170
 III,6-7 183
 V,18 202

4Q261 (*4QS^s*)
 1 2 236

4Q265 (*4QSD*)
 6,2 475

CD (*Document de Damas; Zadokite
 Fragments; Cairo Damascus Cov-
 enant*)
 I,11 183

I,11-12	186	4Q271 (<i>4QD^f</i>)	
I,21	244	4 ii 13	235
II,2-13	170; 252	5 i 18	253
II,14-15	186		
II,15	189	4Q274	
III,13-14	170	1 i 5-6	369
III,14	171; 178		
IV,8	178	4Q298	
V,1-6	194	1 2	372
V,3-4	196	1-2 i 3-4	264
V,5	195		
V,5-6	197	4Q299 (<i>4QMyst^e</i>)	
V,17-18	253	1 1	202
V,20	181		
VI,14	178	4Q305	
VI,18	178	<i>Ensemble</i>	538
VI,20	178		
VI,7	172; 179; 184; 185	4Q318	
VI,7-11	185	VIII 9	571
VI,9-11	183		
VII,18	173; 179; 185	4Q319	
VII,18-21	67	<i>Ensemble</i>	538
X,20-21	244		
X,21	479	4Q327	
XI,20-21	244; 189	<i>Ensemble</i>	157
XII,2-3	253		
XIII 1	56; 571	4Q364	
XIII,5-6		28 3	58
XVI,2	180		
XVI,13	235	4Q365	
XX,6	173; 176	23	429-430
XX,28.32	186	23 7	235
		4Q366	
4Q266 (<i>4QD^a</i>)		4 i 7	235
1a-b 1	253		
2 i	179	4Q368	
2 i 15	170	10 i 6	235
2 i 4-6	171		
2 i 5-6	170; 171	4Q369	
2 i 7	170	1 i 6	257
3 ii 5-6	253		
5 i 17	183	4Q374	
		<i>Ensemble</i>	514
4Q268 (<i>4QD^c</i>)			
1 1-8	194	4Q375	
		<i>Ensemble</i>	171
		<i>Ensemble</i>	182
4Q270 (<i>4QD^e</i>)			
2 ii 19-20	253	4Q377	
7 i 20	183	2 ii 10	49

2 ii 5	571	4Q418	
2 ii 5	571	55 6	380
		69 ii 6	571
4Q378		77 2	255
<i>Ensemble</i>	386	77 3	254
10 2	48	81 10	264
14	44	81 17	252
22 i	45		
26	45	4Q419	
3 i-ii	43-61	<i>Ensemble</i>	538
4Q379		4Q422	
<i>Ensemble</i>	386	<i>Ensemble</i>	263
10	45		
18	45; 59; 60	4Q423	
		5 1	170
4Q385			
16 ii 8	180	4Q427	
		<i>Ensemble</i>	171
4Q392		7 i 20	320
<i>Ensemble</i>	538	7 i 86-23	338
4Q393		4Q433a	
<i>Ensemble</i>	538s	2 5	235
3,8	325		
		4Q438	
4Q397 (<i>4QMMT^d</i>)		<i>Ensemble</i>	171
14-21 10-11	191		
		4Q439	
4Q398 (<i>4QMMT^e</i>)		1 i 1	265
11-13 1-3	52	1 i 2	266
14-17 ii 2	48		
		4Q444	
4Q401 (<i>4QshirShab^b</i>)		1-4 i+5 7	255-259
<i>Ensemble</i>	171		
		4Q448	
4Q405 (<i>4QshirShab^f</i>)		<i>Ensemble</i>	538s
13 6	245		
		4Q462	
4Q414		I,11	518
2 iv 7	235		
		4Q463	
4Q415		<i>Ensemble</i>	171
11 9	571		
		4Q471b	
4Q417		<i>Ensemble</i>	338
1 i 15.	571		
2 i	203	4Q472a	
2 i 6	179	<i>Ensemble</i>	261-268
		1-5	483ss

2-4 475

4Q487

24 251

37 251

4Q491 (*4QM^a*)

1-3 10 56

11 i 8-24 338

11 i 12-18 339

10 ii 14 57

4Q496 (*4QM^f*)

Ensemble 538

4Q501

3 236

4Q502

16 251

4Q504 (*4QDibHam^a*)

1-2 iv 7 571

XIX,5-6 55

4Q505

Ensemble 538

4Q506

Ensemble 538

4Q508

Ensemble 171

4Q509

Ensemble 171; 538

4Q510 (*4QShir^a*)

1 9 245

4Q511 (*4QShir^b*)

28-29 4 256

48-49+51 ii 3-4 256

4Q512

Ensemble 171

4Q521

2 ii 1-15 313-340

2 iii 6 67

4Q522

Ensemble 386

1 60

7 60

8 44; 60

9 i 44; 60

9 ii 60; 61

4Q525

11-12 251; 258

15 371-381

15 7 571

4Q531 (*4Qgiants^c*)

19 2 552

4Q541

9 i 3 337

4Q543

1 558

4Q545

1 558

4Q557

1 5 545

4Q562

2 4 497

4Q569

1 2-8 544

5Q9

Ensemble 44; 45; 387

5Q13

Ensemble 171

1 9-13 171

5Q15

1 ii 571

5Q16

Ensemble 371-381

6Q8

3 2 572

6Q15		XVII,6-9	359
5	253	XVII,8-9	361
5 5	571	XVIII,8	202; 203
		XIX,8	202
6Q30		XXI,3	361
1 ii 5	264	XXI,9	202
		XXII,9	362
8Q3 (8QPhyl)		XXII,13	362
<i>Ensemble</i>	88ss	XXII,14	202; 203
I	79	XXIV,8-9	203
		XXIX,5	235
11Q5 (11QP ^{s^a})		XXV,8	202; 203
<i>Ensemble</i>	460-464; 538	XXVII,4-5	203
III,5	217	XXVII,5	202
VI,12	217	XLII,10-17	360
XIV,5	217	XLII,13-14	155
XVIII,12	324	XLIII,12-14	360
XIX,15	342	XLV,7-14	367
XXI,11-XXII,1	207-231	XLV,11-12	360
XXIII,6	219	XLV,12-14	244
XXIII,15	217	XLV,15-18	367
XXVIII,9	216	XLVI,13-16	475; 477s; 483; 565
		XLVI,16-18	367s
11Q6 (11QP ^{s^b})		XLVII,3-14	363
<i>Ensemble</i>	463s	XLVII,7-10	364
		XLVII,14-17	364; 367; 368
11Q8 (11QP ^{s^d})		XLVIII,17	364
1	529-531	XLIX,12	192
		XLIX,13	375
11Q10 (11Q ^{tg} Job)		LII	191
<i>Ensemble</i>	538	LII,13-16	360
XXVI 5	572	LII,15-17	364
		LII,19-21	364
11Q11 (11QP ^s Ap ^a)		LIII,9-10	364
<i>Ensemble</i>	538	LIV,5-7	189
		LIV,6-7	202
11Q17		LVI,1-11	182
<i>Ensemble</i>	538	LVI,4	174
		LVI,21	175
11Q18		LVII,4	56
<i>Ensemble</i>	538	LIX 7	50
		LIX 7-8	49
11Q19		XQPhyl	
(11Q ^{Temple^a} ; Rouleau du Temple)		1	
<i>Ensemble</i>	538	<i>Ensemble</i>	88ss
VIII,13	202	2	
IX,14	202	<i>Ensemble</i>	88ss
XIII,16	202	3	
XIII-XXIV	429	<i>Ensemble</i>	88ss
XIV	158		
XVII,4	202		

Vision de Gabriel		1,27-29	337
<i>Ensemble</i>	491-524	2,1	337
		3,31	33
Autres textes du désert de Juda		4,5	33
<i>Na ḥal Hever</i>		5,13	33
<i>Ensemble</i>	538	6,17	33
<i>Masada</i>		6,28-38	33
<i>Mas 11</i>		6,31	33
<i>Ensemble</i>	387; 539	6,32-38	171
<i>Mas 1a</i>		6,33-38	35
<i>Ensemble</i>	539	6,35	33
<i>Mas 1b</i>		15,25	33
<i>Ensemble</i>	539	16,9	33
<i>Mas 1</i>		16,28-29	33
<i>Ensemble</i>	539	18,19	33
<i>Mas 1d</i>		19,9	33
<i>Ensemble</i>	539	21,12-14	160
<i>Mas 1e</i>		21,15	160
<i>Ensemble</i>	539	23	72
<i>Mas 1f</i>		23,32	33
<i>Ensemble</i>	539	24,28-29	65; 66; 71
<i>Mas 1h</i>		27,13-18	160
<i>Ensemble</i>	539	28,6	33
<i>Mas 1k</i>		30,2	545
<i>Ensemble</i>	539	30,3	545
<i>Mas 1m</i>		30,4	545; 549s
<i>Ensemble</i>	533-542	30,5	545; 551
<i>Mas 1o</i>		30,6	553
<i>Ensemble</i>	539	30,7-18	560
<i>Mas 1p</i>		30,9	33
<i>Ensemble</i>	539	30,19-20	33
<i>Mas 1k</i>		31,32	33
<i>II,22</i>	255	32,4-16	360
<i>MasPs^b</i>		32,14	362
<i>Ensemble</i>	464	32,15	33
		32,21-22	33
Références aux Apocryphes et autres écrits		33,10	33
		34,3	549
		37,6	72
<i>I Hénoch</i>		37,10	65; 66; 71
<i>1-11</i>	257	42	72
<i>33,3</i>	33	46,9-10	162
<i>72-82</i>	3-41	49,8	33
<i>90,6-12</i>	326	49,16-20	362
<i>91,11</i>	326		
<i>92,1</i>	33		
<i>106,19-107,1</i>	33		
<i>108,6-7</i>	33s		
		<i>Testament des Douze Patriarches</i>	
		<i>Test Lévi</i>	
		1,1-2	558
<i>Jubilés</i>		13,9	555
<i>1</i>	72	2,5	554

5,1	554	Cambridge col. c	
5,3-4	551	16	549
6,2-5	543		14 551
6,3	545s		16 551
6,4	549		22 551
6,4-5	550s		
6,6	552	Cambridge col. d	
6,7	552		17 550s
6,8	551		22 551
7,3	551		18 551
18,3	67		19 552
		20	553
<i>Test Asher</i>		21	553
2,10	33		22-23 553
7,50	33		
		Cambridge col. f	
<i>Test Juda</i>		1-16	555-557
4,1-3	554		
24,1-6	67	<i>Psaumes de Salomon</i>	
		17,2	68
		2,3	330
		2,34	330
<i>Testament de Lévi</i> araméen de la		2,35	332
Geniza du Vieux Caire		2,36	330; 332
		3,8	330
Bodleian Library col. a		3,11-12	330
1	549	3,12	322; 333
7	559	4,1	330
		4,8	330
Bodleian Library col. b		4,9	325
1	549	4,12	330
23	549	6,6	332
		6,8	330
Bodleian Library col. c		7,7	332
7	549	8,11-13	330
22	549	8,23	330
		8,27	332
Cambridge, col. a		8,28	334
15	544s	8,31	334
	16 545	8,34	330
	17 546; 549	9,3	330
	18 547	9,8	332
	19 547	9,10	334
	20 547	10,6	330
	21 547	12,4	330; 331
	22 547	12,5	325
	23 547	12,6	330
		13,6-12	330
Cambridge col. b		13,10	330
15	549	13,12	330; 332

13,14	330
14,3	330
14,10	330
15,1	332; 334
15,6-13	330
17	332s
17,2-3	334
17,4-7	331
17,5-10	328
17,7	330
17,15	325
17,16	324s; 330; 335
17,21	331
17,34	334
17,39	334
17,43	337; 338
18,2	334

Eusèbe de Césarée*Préparation évangélique*

VIII,11.13	238
IX 21,9	550
IX 22,8-9	551
IX 22,10	550

Constitutions Apostoliques

<i>Ensemble</i>	558
-----------------	-----

Références à Philon d'Alexandrie*De Praemiis et Poenis*

95	67
----	----

Pseudo-Philon

Liber Antiquitatum Biblicarum

8,7	550
-----	-----

Références à la littérature classique

Tacitus

Historiae

V,13	68
------	----

Suetonius

Vespasianus

4	68
---	----

Références à la littérature mésopotamienne*Enuma Anu Enlil*

XIV	21-29
-----	-------

Références à Flavius Josèphe*Guerre des Juifs (BJ)*

II,119-166	86
II,147-149	475; 479
II,591-592	423
III,41-44	424
III,43	426
III,46	426
III,49-50	426
III,516-521	426
V,227	369
VI,312-313	68; 70
VII,253	86
VII,375-380	86

Antiquités Judaïques (Ant)

I,128	65; 66; 70; 74
I,339-341	552
I,340	550
III,261	369
IV,212	318
IV,218	175
IX,74	369
IX,226	369
XII,395	326
XIII,3	426
XIII,301	68
XV,108	426
XV,299-300	426
XVIII,23	86
XX,16	317
XX,51-53	426

Contra Apion

I,281	369
II,103	369

Vita

45	426
----	-----

Littérature targumique*Tg Neophyti**Gn*

25,31	546
-------	-----

*Tg Onqelos**Dt*

7,6	518
14,2	518
26,18	518

Littérature rabbinique

Bérakhot

Mishnah

2,1 318
2,3 180
4,3 178
5,1 321; 332

Bavli

9b 321
28a 565
34b 188
60b 321
61b 565
62a 565
Tosefta
2,1 319

Ma'asser Šéni

Mishnah

5,3-4 501

Šabbat

Bavli

74b 513

Érouvin

Bavli

18b 512
55b 199

Péssahim

Bavli

39a 512
80a 190

Šekalim

Yerushalmi

49b 504

Yoma

Mishnah

5,3 501
5,4 501
Bavli
75b 199
80a 189; 202

Souka

Bavli

46a 507

Taanit

Yerushalmi

68d 68

Méguila

Yerushalmi

1,8 496

Nazir

Mishnah

3,6 504

Sota

Tosefta

7,18 195
13,1 195

Guitine

Bavli

56b 512
Yerushalmi
49b 265

Baba Batra

Bavli

14b 196
81b 365

Sanhédrine

Bavli

17b 565
24a 512
111b 501
Tosefta
7,1 204

Makot

Mishnah

5,10 512

Avot

Mishnah

1,1 176; 194
1,4 323

Zéva'him

Mishnah

5,8 358
Bavli
112b 199
55a 199

<i>Tosefta</i>			<i>Lev Rabbah</i>	
13,3	199		14,4	355
<i>Ménahot</i>			<i>Nb Rabbah</i>	
<i>Mishnah</i>			16,24	565
3,7	82		<i>Sifre Nombres</i>	
<i>Bavli</i>			<i>pisqa</i> 1	198
29b	202		<i>Sifre Deutéronome</i>	
34b	82		34	82
80b	365		<i>Sipre Nombres</i>	
<i>Houline</i>			1	368
<i>Bavli</i>			<i>Sipre Deutéronome</i>	
130b	365		75	361
<i>Témoura</i>			<i>Pirqè de Rabbi Éliézer</i>	
<i>Bavli</i>			288	550
15b	204		<i>Midrash Tehillim</i>	
<i>Tamid</i>			Hab 2,2-4	188
<i>Mishnah</i>			<i>Mekilta d-rabbi Ishmael</i>	
5,1	318		bo, <i>parasha</i> 12192	
7,4	317		<i>Kallah Rabbati</i>	
<i>Midot</i>			5,5	501
<i>Mishnah</i>			80,4	501
1,3	493		80,6	501
4,6	200		<i>Tefilla</i>	
4,7	200		Deuxième bénédiction	313-340
<i>Kélim</i>				
<i>Mishnah</i>				
1,8	358			
<i>Tosefta</i>				
1,12	198; 368			
<i>Gen Rabbah</i>				
65,22	323			